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FEVER AND CHOLERA.

FEVER AND CHOLERA

FROM

A NEW POINT OF VIEW.

BY

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[FOR PRIVATE CIRCULATION.]

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PREFACE.

TWELVE years ago, the writer of the following Essay first began to collect materials for it; and, from the year 1861, till the present time, the subject of the task which he has just completed has never been altogether lost sight of.

It was only, however, within the last three years, that the very important bearings flowing from a more extended consideration of its various relations, became clear to his mind; and he is now glad to be in a position to place his views before the public in what he trusts may be considered a reasonably complete form.

The writer has to beg that his Essay may be viewed purely in the light of a private and gratuitous contribution to Medical Science; and that, therefore, as it

in no way partakes of an official character, it may be distinctly understood that he alone is to be held responsible for the opinions and recommendations it contains.

Had the publication which the writer is about to issue been of a character worthy of being dedicated to any one, he would certainly have taken the liberty of addressing it to his former teacher, Sir Robert Christison. As it is, he has only to add the expression of his sincere conviction, that should it prove the means of advancing any view, or of supplying any recommendation, possessed of real practical value, that consummation will be mainly owing to his steady endeavour to keep in view, and be guided by, the principles which his distinguished preceptor has, for many years past, so ably inculcated.

The writer hopes that, by the distribution of 250 copies of his Essay among those most likely to be interested by it, both in India and in England, the object for which it was written will have been attained; but should subsequent events

show its more extended circulation to be desirable, measures will, without loss of time, be taken to place a public edition within reach of the profession generally.

United Service Club,
Calcutta, January, 1873.

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FEVER AND CHOLERA

FROM

A NEW POINT OF VIEW.

CHAPTER I.

GENERAL AND ETIOLOGICAL.

SOME forty years have, the writer believes, now elapsed since Sir Robert Christison, in treating of the general doctrines of fever, wrote the following words (see page 122, vol. I., "Library of Practical Medicine") :—"The errors of pathologists in investigating the question of the primary or secondary nature of fever seem to have mainly arisen from a circumstance, to which several other important errors that have gained currency are equally owing, namely, from limited observation, extensive enough perhaps in one sense, but limited in so far as it comprised experience of the phenomena of the disease in one locality only, and often too in one epidemic of that locality."

The writer feels constrained to express his

belief, that the more carefully he considers the state of medical opinion on this all-important question, as that is demonstrated by the labours of contemporary authors, the more fully does the conviction force itself upon him that the remarks of the distinguished observer just quoted are but little if at all less applicable to the efforts that are now being made to throw light on the nature of fever, as the matter stands at the present day, than they were pertinent to the condition of things that existed in this respect when the words quoted above were originally penned. For, whilst the field of investigation may have been extended, in the sense that fuller records of facts are being made over a much wider geographical area, it is yet by no means clear that the observers who have really made their voices heard in connexion with this inquiry, have not now, as formerly, had their powers too exclusively directed to what was happening in one particular section of the subject under investigation, without their having been sufficiently careful to compare, in a more largely general way, the intimate relations by which the whole series of the phenomena of fever are linked together. So much has this been the case, that it has come almost to appear as if the aim of each succeeding investigator had been to isolate as completely as possible, and give an

importance which could not, in point of fact, be justly claimed for it, to some one special form into which this disease is at times, and under varying circumstances, apt to develop itself—whether this may be in relation to the manifestation of particular symptoms, or with reference to the presence of peculiar pathological anatomical appearances.

The writer hopes to be able to show in the sequel, that all observations of any real value made within the last forty years, have only tended to prove the accuracy of the following general principles as to the nature of fever, enunciated at page 123 of the article already quoted from, and which will be taken as the text of what has now to be written:—"The first appreciable event in the chain of sequences constituting fever is a functional injury of the nervous system. The only essential or invariable consequence of this affection is functional derangement of most of the important organs of the body, but more especially of the brain, the circulating organs and fluid, the alimentary canal, and the skin."

In order, however, to endeavour to show, from evidence collected since the above quotation was written, how true to nature the principles which it lays down are, and, at the same time, to attempt to make clear the important lessons in pathology and therapeutics which a further consideration of

those principles in all their bearings teaches, in respect of some of the most important diseases to which the human race is liable, it will be necessary—

Firstly, to take a wide view of some of the more prominent etiological considerations which affect non-contagious febrile diseases generally.

Secondly, to trace the points of resemblance which exist between all such complaints; and, moreover, to adduce the facts that go far even to prove their actual identity, when due allowance is made for varying severity of individual attacks, and for peculiarities of constitution in those suffering from them,—the evidence on this subject being, in point of fact, such as to make it extremely probable that those diseases are merely varieties in degree of the same complaint.

Thirdly, to show how strong proofs there are that the disease known as cholera is neither more nor less than a member of this group expressed in the highest degree of intensity in which any one belonging to it has yet manifested itself.

And, lastly, to deduce the important practical teachings which the development of the foregoing conclusions will have led up to when the matter is regarded in relation to army and general hygiene, and to therapeutics.

In his endeavour to accomplish the task he has

set himself to perform, it appears to the writer that he cannot do better than strive earnestly to lay to heart the lesson which the following still further remarks by Sir Robert Christison contain. They are to be found at page 114 of the work already referred to, and are not less remarkable for their force of diction than for the accuracy with which, after an interval of nearly half a century, they continue to be applicable to the state in which, if we except what has been done by Sir Robert Christison himself, this very question of the general doctrine of fever still remains :—

"But all that has been attempted in this particular corner of the theory of fevers since Dr. Cullen wrote, has had scarcely any other tendency than to embarrass and obscure the subject. Much has been done for the pathology of the various forms of fever by the researches of pathological anatomists; and the knowledge now possessed of its secondary affections has in consequence acquired an extent and precision, which were in his days unknown and indeed unattainable. But the views entertained of fevers in the abstract have not been rendered clearer; and many dogmas have been maintained, and still receive currency, which, in so far as they do not seem true to nature, cannot be supposed likely either to advance medical theory, or to benefit medical practice."

It will only be further added here, by way of preface, that it is hoped the opportunities for observation which an active service of now over a quarter of a century as an executive army medical officer, in every variety of climate; in England, in Ceylon, and in the Crimea; in Canada, and in India, has afforded the writer—not forgetting also the advantages that during the last three years of his service he has had the good fortune to enjoy whilst in charge of the Statistical Department of the British Forces in Bengal—may be considered no small justification of the act of presumption, which his venturing to advance opinions so much at variance with those which are, at the present time, of almost universal prevalence, will, probably, at first sight appear to be.

It is confidently expected, however, that, when the evidence now about to be adduced shall have been duly considered, it will be found possible, in at least some measure, to exonerate him from so grave a charge.

It has long appeared to the writer that the attainment of a more complete understanding of the nature of many important diseases of the class now under consideration, has been very materially retarded by the belief which has gained currency, that the fevers and other allied diseases of tropical climates differ, in some essential particulars

besides such as are the result of degree of intensity alone, from complaints of a similar nature occurring in temperate latitudes.

This belief the writer's own observation would lead him to consider altogether destitute of foundation on fact.

It is only necessary to look back to the times and writings of our early English medical celebrities, and to the records left by many distinguished authorities subsequent to their day, to find how prevalent agues and remittent fevers were in England at a former period, and how little they differed, in any essential respect, from those diseases as they occur at the present time in tropical climates.

Thanks, however, to the advances made in agriculture, and, within the last half-century, to the sanitary laws brought to bear on the health of towns; to the introduction of subsoil drainage; to the reclamation of waste lands; and to the great improvements that have taken place in the social condition of the labouring classes within the British Isles, those diseases have become much less frequent than they were formerly, whilst their types have, at the same time, become milder. Nevertheless, the writer would here affirm his belief, founded on his own experience in regard to fever in various forms as it occurs both in tropical

and temperate climates, that, judging from what he has seen whilst in charge of troops there, much fever still occurs in the climate of Great Britain, more especially in the southern and eastern parts of England, at certain seasons of the year, of an intermittent and remittent character, which differs from that so ably described by the older writers only in its being now of a milder type—a change for the better which has probably been, in a great measure, brought about by the social improvements enumerated above.

Even in India, although, owing to extra intensity of exciting cause, the type of those fevers presents greater severity of symptoms than is usually met with in Great Britain, yet, in certain seasons, and under like attending conditions, the types of fever of the latter country may be sufficiently closely matched to satisfy any one of the radical identity of the two forms, notwithstanding that those occurring in England are now, as a rule, returned under the heads of "simple continued" and "enteric" fevers.

That, also, diseases identical with as to actual symptoms, and almost equalling in virulence those of tropical climates, as met with there at the present time, may occur in temperate latitudes during the heats of summer, will be at once seen on referring to Sir John Pringle's work on the

diseases of the army employed in Flanders about the middle of last century.

That writer's graphic descriptions of disease, which bear in their face the stamp of being admirably true to nature, show that, on the continent of Europe more than a century ago, just as happens at the present day in different parts of India, the conditions of locality, of employment, of the climate of a particular year, of exposure, &c., regulated not only the amount of disease attacking bodies of men, but also, then as now, determined the times of access and decadence of those diseases in respect of the seasons of their epidemic prevalence, as well as the forms they assumed, and the degrees of intensity in which they manifested themselves.

Sir John Pringle's observations are now so little known, whilst they are, at the same time, so applicable, in an etiological sense, to the diseases of which he treats as those are witnessed at the present time, that the writer feels convinced, that, whilst advancing his own arguments by so doing, he will confer a boon on many of his readers by quoting some of the more striking passages of a book replete with valuable information, but which, he fears, is now known to comparatively few members of the medical profession.

In his work on the diseases of the army

serving in Flanders, Sir John Pringle begins by stating, generally, that the more elevated and drier parts of that country were healthy; that its low and wet portions, as well as the greater part of the united provinces, with Dutch Brabant from Grave downwards along the Maes, were sickly; whilst the province of Zealand, which was not only similar to the parts above-named in regard to lowness of level and the moisture of its soil, but was also surrounded by the oozy beaches of the Eastern Scheld, and the most marshy parts of the whole country, was the sickliest of all.

After remarking, further, that this part of Flanders, from its being much exposed to inundations, and from being also under the level of the sea and rivers, was liable to have its atmosphere loaded with moisture, and with putrid and unhealthy vapours, he adds (at page 2) the following remarkable passage, which the writer cannot help believing that few who read these pages will peruse without at once detecting the origin of the so-called "Pettenkofer-theory." Should they, however, fail to do this, they will at all events learn, that, presuming the theory in question to have been advanced in ignorance of Sir John Pringle's observations, the latter was undoubtedly the first to establish the principle on which it is based, and to him therefore is justly due the credit of having

originated what has since, virtually, been reproduced as a fresh discovery of much importance:—

"A second but less obvious source of humidity is from the water under the ground, which, in that country, is so near the surface that a dry ditch is seldom seen; and as the soil is light, the moisture easily transpires, and in summer loads the air with vapour, even when no water is visible. This is the condition of much of Dutch Brabant, where the people are more or less subject to intermittent fevers in proportion to the distance of this water from the surface, so that, by looking into their wells, one may form a judgment of the comparative healthfulness of the several villages: for these wells, being fed by the subterraneous water with which they are on a level, and sinking in proportion to the droughts of summer, are a proof of the constant exhalations of the concealed moisture through the pores of the earth by the heat of the sun."

Again (at page 4):—"This is the nature of the country. But, according to the various degrees of heat and moisture of the season, the epidemic diseases begin earlier or later, are of longer or shorter duration, and are attended with milder or more alarming symptoms. When the heats come on soon and continue throughout the autumn, not moderated by winds and rain, the

season proves sickly, the distempers appear early, and are dangerous. But when the summer is late, or tempered by frequent showers and winds, or if the autumn colds begin early, the diseases are few, their symptoms mild, and their cures easy: and here it may be proper to distinguish between the moist and rainy seasons; for in marshy grounds, intense and continued heats, even without rain, occasion the greatest moisture by the exhalation which they raise and support in the atmosphere; whereas frequent showers during the hot season cool the air, check the rise of vapours, dilute and refresh the corrupted water, and precipitate the noxious effluvia. But if heavy rains fall in the beginning of summer, and are followed by great and uninterrupted heats, then the water collected by the rain, stagnating in the lower grounds and corrupting there, furnishes matter for more exhalation, and thereby renders the seasons more sickly, and the diseases more fatal.

"It ought also to be remarked, that the sickness never begins till the heats have continued long enough to give time for the putrefaction and evaporation of the water. The epidemics of this country may, therefore, be generally dated from the end of July or the beginning of August, under the canicular heats; their sensible decline, about the first falling of the leaf; and their end,

when the frosts begin; the rest of the year is much less disposed to produce any disease."

When it is borne in mind that the above quotations refer to diseases (intermittent and remittent fevers) which occurred on a part of the continent of Europe situated but a few miles from the shores of England, lying between the parallels of latitude which include the portion of the latter country extending from Canterbury on the south to Colchester in the north, and possessing a mean annual temperature rather below that of the corresponding part of England,—although, from its continental position, its summer temperature is somewhat higher,—the force of the writer's proposition as to the identity of the non-contagious febrile diseases met with in different parts of the world, in respect of the uniformity of action of the laws which regulate their occurrence and development, whether in temperate or in tropical climates, when what happens in the former, in exceptional seasons,—and, indeed, to some extent, in all seasons,—is compared with the manifestations in this respect witnessed in the course of ordinary seasons in the latter, can scarcely fail to be admitted by those who have had extended experience in such matters in both sets of climates, when they compare that which they may have acquired at the present day with the records

of the past. Due allowance must however be made, in all such comparisons, for the effects of the collateral modifying circumstances which always exist under such widely varying conditions, and which never fail to regulate the degrees of intensity (in other words, the "types") in which those complaints manifest themselves.

In order still further to attempt to make it clear that a condition of things almost identical with what has been so graphically described by Sir John Pringle as having occurred many years ago in a temperate climate, repeats itself, year by year at the present day, in those parts of India where the seasons are sufficiently marked to ensure a "cold-weather" period of some months' duration, the following general details are here offered :—

1. The hot and dry months of early summer in India bring with them fevers of every form met with in that country. The larger proportion of those, however, partake of what is known as the "ardent" type, are attended with unusually marked cerebral symptoms and high fever, and often run rapidly on to a fatal termination. In some cases this latter occurrence takes place by the ordinary coma of fever showing great determination to the head ; in others, whilst coma is still the fatal symptom, it is coma manifested at the outset of the hot stage of the disease, either in an

original attack showing itself under circumstances involving much exposure or great bodily fatigue, or occurring, as often happens in the corresponding stage of an exacerbation of fever, whilst the subject of it is under treatment in hospital. Whatever the period or circumstances of the accession of this mode of fatal termination may be, it, as a rule, shows itself in a way so appallingly sudden, as to have led to such cases being taken out of the category of ordinary fever, and returned indiscriminately (and that, too, in a large proportion of cases, the writer cannot but think, with far from strict pathological accuracy) under the heads of "sunstroke" or of "heat apoplexy."

2. With the setting in of the rains in June or July, the fevers assume more of the low remittent and continued types, running a much slower course, and showing in an unusually marked degree, especially in the latter form, in a majority of instances, the bowel complications held by medical writers to be characteristic of so-called "enteric" or "typhoid" fever.

This manifestation of the remittent and continued types of fever becomes intensified during the periods of lull which so often happen in the rains, and in which a hot, steamy atmosphere prevails; but it is more especially remarkable during that trying interval that occurs between

the cessation of the rains and the commencement of the cold weather, when the drying up process is going on with greatest activity, and during which, to a greater extent than happens at any other season, the more marked forms of those varieties of fever are prevalent.

It is at those periods also, namely, towards the close of the rains, and from that time till the cold weather has fairly commenced (which is also that portion of the year when the power of the European constitution to withstand influences tending to disease is reduced to its lowest ebb), that outbreaks of cholera are most frequent—that disease appearing then merely as the highest expression of form in which non-contagious fever manifests itself, and that, too, as will be more fully explained further on, almost entirely through the exaggeration of one of its special features occurring at the outset of the attack.

3. Lastly, with the early cool months of the year, corresponding in some respects to the autumnal ones of our English climate, comes the change of type of the prevalent fever to the milder intermittent form. This is still, however, attended by an admixture of the more severe varieties, and especially of cases showing marked enteric lesions. The condition of things last noted is followed, from about the middle to the close of the cold weather,

(and even for some time after the latter has begun to shade off into the earlier warm months,) by the decadence in intensity, both as to severity of symptoms and frequency of occurrence, of all the forms of febrile disease now under consideration, and the attainment of their minimum epidemic prevalence among Europeans reached in the course of the year.

It ought at the same time, however, to be explained that it must not be inferred from what has just been stated, that the improvement in health, as regards sickness caused by fever, begins everywhere with the accession of the cold weather. On the contrary, it has become a well-recognized fact, that, at all stations where much fever may have prevailed during the hot months, and more especially in the upper provinces, where, as a rule, the transition from the hot to the cold season is marked by great alternations of temperature between day and night, there is, at first, a very marked increase of disease, generally in the form of ague, in those soldiers who have suffered the most from fever in any form during the hot weather, and who may, in consequence, have fallen into a low anæmic state. There can be little doubt that this is mainly owing, among troops, to sudden changes of temperature—cold, experienced under such circumstances, acting as

a potent agent in inducing relapses in those who may already have suffered from fever.

Deficiency of covering for the men at night, at stations where cold is most felt, has also, probably, had much to do with this source of disease. The necessity for remedying this defect has now, however, been fully recognized and acted upon, by the military department of the Indian Government, as an important hygienic measure.

All past experience seems to show very clearly, that, until the bracing power of the cold weather has had time to assert itself by increasing the general tone of the human frame, the first effect of the cold, dry atmosphere which prevails at the outset of the cold season in Upper India—and which conveys to the body a sensation of cold of an intensity such as can only be understood by those who have actually experienced its effects—is, by inducing severe chills, to lead to relapses of fever in men previously debilitated by that disease, and even to excite the complaint, for the first time, in those who may have become prone to its attacks, although they may, hitherto, have escaped an actual seizure.

Before closing this section of his subject, it appears to the writer to be desirable that he should state the conviction, long present to his mind, that

one very potent cause of hinderance to any real advance in our knowledge of the nature of this class of diseases, has been that which has had its origin in the confusion occasioned by the use of the terms "malaria" and "marsh miasmata" in a sense implying that those modes of expression indicate, in any respect, the existence of two distinct sources of disease.

Extended observation on this subject tends only more clearly to show, the more the matter is looked into, and the more closely the results which follow exposure to the different disease-producing conditions to which, in the absence of more definite information as to their nature, the term "malaria" has been applied, that this factor of disease differs only in degree of intensity, whether the conditions to which it owes its existence proceed from the surface or sides of a marsh properly so called, or from ground in a swampy state; from ordinary soil under usual conditions in a tropical climate (or in a temperate one during warm weather,—and, indeed, at other times as well); or from that which has, in any climate, been rendered foul by animal impurities or vegetable decay; from ground drying up after rain under a tropical sun, or which, being of a porous nature and outwardly apparently dry, may yet contain much sub-soil water, within a short distance of its

surface, of which the open nature of the soil may permit the ready evaporation.

To repeat, whether proceeding from any of the above-named sources, or from sewer or cess-pool effluvia—both of which last-named forms of emanation, judging from the results which have followed exposure to their influences, are disease-causing media similar in action to those proceeding from other sources,—the effects produced by malaria (using this convenient term in its most extended sense) are identical in the essential features of the results they induce, however much those may be modified, in respect of intensity and the varying shades of expression which that leads to, by the atmospheric conditions under which they may have been generated, and according as the states of health, of vital force, and of temperament of body of the human frames they may have acted upon, were favourable or otherwise to the development of the effects they are capable of producing.

The varying terrestrial conditions under which this cause of disease may develop itself, were first fully pointed out by Sir John Pringle, and by Dr. William Fergusson of the British Army Medical Department, many years ago, in a manner so complete as to have left but little to be desired in that respect, although, as has become but too common at the present time, in respect of the

work done to our hands by our predecessors, the labours of those two unrivalled observers have been apparently to a great extent overlooked.

But, whilst speaking of the terrestrial origin of malaria-producing conditions, it must not be forgotten that results similar to those noticed on shore arise also on board ship; and more especially in war vessels carrying coals, and remaining for a time on tropical stations with full crews, foul bilges, and, it may be, with decaying timbers.

In vessels so circumstanced, the results which usually follow what is spoken of as malarial poisoning on shore, have, under conditions favourable to their development, been witnessed in a form so intense as to have been rarely surpassed on land, and such as would, probably, often have been still more disastrous as to results, were it not for the facilities which those vessels enjoy of being able, without delay, to make for cooler latitudes or more healthy localities within the limits of their stations.

The causes of disease proceeding from the various sources enumerated above, exist, there is strong reason to believe, in varying degrees of intensity, in all climates, temperate as well as tropical, whenever a sufficient number of conditions favourable to their development concur; and it would appear that they are to be met with,

everywhere, under varying conditions of the earth and atmosphere.

If, therefore, it is borne in mind that, practically, on this view of the matter, and judging from what little we know of its laws by watching the effects it produces, only one description of so-called malaria exists; that it is produced in varying degrees of intensity under different conditions; and, also, that no country yet known is without some of the terrestrial conditions in varying degrees of intensity from which it springs,—provided, always, other circumstances favourable to its development concur, and there exist subjects prone, from whatever cause, to be impressed by it,—the mode of production of non-contagious febrile diseases becomes, in some respects, more easy of explanation; or, at all events, its investigation may be approached with a far more reasonable prospect of ultimate success, inasmuch as the field of inquiry will thus have been rendered, to some extent, more circumscribed and manageable.

CHAPTER II.

PATHOLOGICAL AND PHYSIOLOGICAL.

In order to attempt to show, still more clearly, the degree of identity that exists between the various forms of non-contagious febrile diseases, the writer will now endeavour to set down the lesson on this matter, which is to be deduced from the study of the leading features of 92 cases of fever which terminated fatally, among European troops in the Bengal Command, between the 26th March and the 21st December, 1871,* and which he has selected from among the records of a larger number that have passed through his hands, on account of the completeness of the histories preserved in respect to the symptoms manifested

* In checking over the materials from which the necessary details and calculations were prepared, it has been discovered that two deaths from enteric fever, which happened in March and May, 1870, have been inadvertently included with the deaths for 1871. As, however, both were average cases of the disease; as full records of them had been preserved; and as their removal from the calculations already completed would have entailed much labour, whilst their retention will in no way invalidate the general bearings of the evidence adduced, the cases in question have only been removed from tables Nos. 7 and 8, which show the distribution of certain diseases over the months of the year 1871-72.

during life, and the pathological appearances observed after death,—defects having been found to exist in many of the other casualty reports which rendered them of comparatively little value towards contributing materials for this inquiry.

The cases that occurred previous to the 26th March, 1871, have not been taken into account, as it was only from that date that an effort made to secure more careful necrological records by the introduction of a new form of casualty report (which, it is hoped, may in time be the means of ensuring the collection of much valuable information respecting the more important diseases of India), came into full operation.

The nomenclature which has been adopted, is, in every instance, that employed by the medical officers under whose charge the cases came during life, and by whom, or under whose immediate supervision, the examination of the bodies was made after death.

The 92 cases in question were made up as follow:—

	No. of cases.
Simple continued fever	13
Remittent fever	49
Enteric „	30
Total ...	92

It is of importance, however, to bear in mind that of the numbers returned as having died under those different headings, a small proportion only of the whole was originally admitted into hospital under the disease recorded as the cause of death; the nomenclature first employed having, in a large proportion of instances, been changed, either in the course of the progress of the disease, or after the diagnosis had been corrected by means of a post-mortem examination.

The extent to which alterations of nomenclature of this description became necessary, is shown below:—

Diseases.	Number of cases remaining throughout unchanged.	Number of cases in which a change of nomenclature became necessary.	Total deaths.
Simple continued fever ...	12	1	13
Remittent fever ...	24	25	49
Enteric „ ...	1	29	30
Total ...	37	55	92

The details illustrating this point for each of the three forms of fever are given below.

Of the 13 cases returned as deaths from simple

continued fever, the original admissions stood as under :—

Diarrhœa	... 1
Simple continued fever ...	... 12
Total	... 13

Of the 49 cases returned as deaths from remittent fever, the original admissions stood as under :—

Bronchitis	... 1
Diarrhœa	... 2
Hepatitis	... 1
Febricula	... 1
Simple continued fever ...	... 4
Ague	... 16
Remittent fever ...	... 24
Total	... 49

And of the 30 cases returned as deaths from enteric fever, the original admissions were as follow :—

General debility ...	... 1
Fever from drink ...	... 1
Simple continued fever ...	... 18
Ague	... 7
Remittent fever ...	... 2
Enteric fever	... 1
Total	... 30

From the particulars which have just been given, it will be seen that the disease returned as

the cause of death was named, at the outset, in 12 out of the 13 cases shown as simple continued fever; in 24 out of the 49 cases ultimately returned as remittent fever; and that in only one instance out of the 30 cases finally set down as deaths from enteric fever, was that disease diagnosed on admission—the correction of the diagnosis having, as a rule, been made after the aid of a post-mortem examination had been obtained.

When, however, the evidence as to their nature, which is afforded by the history recorded of each of the 92 cases, and by the appearances discovered after death, are still more closely scrutinized, it will be found that neither in the 12 cases diagnosed as simple continued fever, nor in the 24 set down from the outset of the disease as remittent fever, was the diagnosis correct, if the symptoms generally held at the present time to be pathognomonic of another form of fever are to be allowed any weight. For, if we are to follow the teaching which, as accepted at the present day, considers every case of pyrexial disease showing a certain combination of symptoms to be an example of so-called specific enteric fever, then no fewer than 68 out of the 92 cases must be set down as examples of that disease, that number having, in addition to signs of pyrexia, shown enteric symptoms, such as gurgling and pain in the right iliac fossa

attended with characteristic diarrhœa,—in fact, the very symptoms which, with or without the presence of an eruption, are held, at home, to be sufficient evidence on which to found a diagnosis of enteric fever, if what has been within the last few years recorded in the periodical medical press is to be taken as indicating the practice in this respect which at present prevails in England. On the other hand, in no less than 89 of the 92 cases there were present some one or more of the following post-mortem appearances hitherto held to be in a great measure characteristic of enteric fever, namely,—1st, enlargement of Peyer's patches and of the solitary glands of the small intestines; 2nd, ulceration of those patches and glands; 3rd, ulceration of the solitary glands of the large intestines; and 4th, enlargement of the mesenteric glands.

If, therefore, all cases showing by their history and post-mortem appearances an average co-existence of the symptoms and pathological appearances noted above, were to be set down as examples of enteric fever,—a course the adoption of which the practice hitherto followed in England would, in one sense, render quite legitimate,—then no alternative would remain but to sweep, almost without exception, from the category of fatal fever, as that disease occurred among British

troops in the Bengal Command, between the end of March 1871 and the close of the year, every form of that disease which did not bear the name of "enteric" or "typhoid" fever.

A glance at the subjoined tables (No. 1, which gives the proportions borne by each of the leading symptoms as they occurred in the 92 cases; and No. 2, which records in a succinct form the principal post-mortem appearances observed in each) will make this clear, and will demonstrate, also, the unexpected fact, that whilst only 10·87 per cent. of the whole of the 92 cases presented what was set down as a true typhoid eruption, no fewer than 51·08 per cent. showed, in a very marked degree, the ulceration of Peyer's patches at the lower end of the ileum, hitherto considered, beyond all other signs, to indicate the true so-called specific enteric fever.

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Table No. 1.

Showing the proportions borne by each of the leading symptoms in the 92 fatal cases of fever.

Form of Fever.	Total of cases.	Head symptoms marked alone.		Enteric symptoms marked alone.		Head and enteric marked combined.		True enteric eruption.		Doubtful enteric eruption.		Albumen in the urine.		Deaths by						Hæmorrhage from bowels.	
														Coma.		Exhaustion.		Could not be classified.			
		Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.		Cases.	Per cent.
Simple continued fever ...	13	2	15·38	4	30·76	7	53·84	0	0	1	7·69	0	0	8	61·53	3	23·07	5		2	15·38
Remittent fever ...	49	22	44·89	9	18·36	18	36·73	0	0	2	4·08	0	0	29	59·18	20	40·81			1	2·04
Enteric ,, ...	30	2	6·66	9	30·00	19	63·33	10	33·33	1	3·33	1	3·33	15	50·00	12	40·00			2	6·66
Totals and percentages ...	92	26	28·26	22	23·91	44	47·82	10	10·86	4	4·34	1	1·08	52	56·52	35	38·04	5		5	5·43

Table No. 2.

Showing the proportions borne by each of the leading post-mortem appearances in the 92 fatal cases of fever.

Form of Fever.	Total of cases.	Congestion of brain and membranes. No effusion.		Effusion of serum within the cranium.		Congestion of mucous membrane of stomach & intestines with or without enlargement or ulceration of glands.		Enlargement of Peyer's patches and solitary glands. No ulceration.		Ulceration of Peyer's patches and of solitary glands, small intestines.		Ulceration of glands, large intestines.		Enlargement of mesenteric glands.		Perforation of intestines.		Enlargement of spleen.	
		Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.	Cases.	Per cent.
Simple continued fever	13	2	15·38	11	84·61	9	69·23	1	7·61	8	61·53	5	38·46	0	0	0	0	11	84·61
Remittent fever	49	8	16·32	32	65·30	38	77·55	9	18·36	13	26·53	4	8·16	5	10·20	1	2·04	39	79·59
Enteric ,, ...	30	4	13·33	17	56·66	27	90·00	3	10·00	26	86·66	7	23·33	7	23·33	2	6·66	15	50·00
Totals and percentages ...	92	14	15·21	60	65·21	74	80·43	13	14·13	47	51·08	16	17·39	12	13·04	3	3·26	65	70·65

Worked out in connexion with each of those heads separately, there is considerable evidence to show that a certain amount of relationship exists between the duration of the disease, irrespective of the name given to it, and the stage at which it may have arrived as regards two of the appearances found after death, namely, whether that shall prove to be simple enlargement of the intestinal glands alone, or whether the condition just named shall have advanced to the stage of ulceration.

The results obtained on this point are as follow :—

First, as to simple enlargement without ulceration,—this occurred in one case of simple continued fever, of which the duration is set down as 18 days; it happened in 9 cases of remittent fever, of which the average duration was 10·55 days, the range in individual cases having been from 3 to 24 days; and it was observed in 3 cases of enteric fever, of which the average duration was 8·00 days, and the range from 5 to 10 days.

On the other hand, there was ulceration of Peyer's patches, and of the solitary glands of the small intestines, in 8 cases of simple continued fever, of which the average duration was 15·50 days, and the range from 8 to 38 days; in 13 cases of remittent fever, of which the average

duration was 17·76 days, and the range from 4 to 70 days; and in 26 cases of enteric fever, of which the average duration was 16·88 days, and the range from 4 to 62 days.

Lastly, ulceration of the glands of the large intestines was observed in 5 cases of simple continued fever, of which the average duration was 13·20 days, and the range from 8 to 18 days; in 4 cases of remittent fever, of which the average duration was 14·75 days, and the range from 6 to 25 days; and in 7 cases of enteric fever, the average duration of which was 17·85 days, and the range from 5 to 32 days.

Information as to the first two points is given in table No. 3.

Table No. 3.

Showing the relation existing between the occurrence of certain pathological states, and the period of duration of the disease in 92 fatal cases of fever.

Form of Fever.	Simple congestion of mucous membrane of stomach and intestines unaccompanied by enlargement or ulceration of glands.			Simple enlargement of solitary and clustered glands of intestines. No ulceration, but with more or less congestion.			Marked ulceration of solitary and clustered glands, with more or less congestion of mucous membrane and enlargement of glands.			Remarks.
	Cases.	Average duration in days.	Range in days.	Cases.	Average duration in days.	Range in days.	Cases.	Average duration in days.	Range in days.	
Simple continued fever	2	6·00	4 to 8	1	18	18	8	15·50	8 to 38	The one almost invariable morbid appearance seems to have been the presence, in varying degrees, of congestion of the mucous membrane of the stomach or intestinal track, with or without enlargement of glands alone, or with some degree of the latter condition mixed with ulceration; the congestion, however, subsiding after considerable advance of the ulcerative process had taken place, and that of reparation was about to set in.
Remittent fever ...	19	12·47	2 to 43	9	10·55	3 to 24	13	17·76	4 to 70	
Enteric „ ...	1	13	13	3	8·00	5 to 10	26	16·88	4 to 62	
Totals ...	22 or 23·19 per cent.	.....	.....	13 or 14·13 per cent.	.....	.....	47 or 51·08 per cent.	.....	.....	

When the question of the relation borne by the duration of the disease to the presence or absence of ulceration, is considered simply in regard to the presence or absence of ulceration of Peyer's patches and of the solitary glands of the ileum, the result obtained is, that no ulceration existed in 45 cases of which the average duration was 11·31 days; whilst of the remaining 47 in which ulceration was present, the average duration was 16·97 days. This is shown in table No. 4 given below:—

TABLE No. 4.

Showing the relation existing between the duration of the disease, and the presence or absence of ulceration of Peyer's patches and solitary glands of the ileum.

CONDITION OF PEYER'S PATCHES AND SOLITARY GLANDS OF ILEUM.	S. c. fever.	Remittent fever.	Enteric fever.	TOTAL.	Average duration in days.	Range in days.
	Cases.	Cases.	Cases.			
Not ulcerated ...	5	36	4	45	11·31	2 to 34
Ulcerated	8	13	26	47	16·97	4 to 70
TOTAL	13	49	30	92	...	...

The spleen, in the majority of instances, was much altered from its normal condition, and, as a rule, it was greatly enlarged. An attempt will be made, further on, to bring out the significance of the varying states of this organ when examining the relationship existing between fever and certain other important diseases. In the meantime, the few facts that could be collected on this subject are given on the next page.

Table No. 5.

Showing the average size of the spleen in certain fatal cases of fever, as compared with its condition in cases of cholera and dysentery.

Form of Disease.	Fatal cases of fever in which the occurrence of diarrhœa is not named.			Fatal cases of fever attended with marked diarrhœa.			Cases of cholera.			Cases of dysentery.		
	No. of cases.	Average size of spleen in oz.	Range, oz.	No. of cases.	Average size of spleen in oz.	Range, oz.	No. of cases.	Average size of spleen in oz.	Range, oz.	No. of cases.	Average size of spleen in oz.	Range, oz.
Simple continued fever ...	2	19·05	5 to 34	10	15·10	8 to 26	...	...	...	...	...	...
Remittent fever	22	12·31	7 ,, 44	21	16·03	6 ,, 40	...	...	...	...	...	...
Enteric ,, ...	2	9·00	8 ,, 10	16	14·06	5 ,, 32½	...	..	...	...	...	...
Cholera	...	...	...	...	...	...	7	5·71	4 to 7	...	...	...
Dysentery	...	...	...	...	...	...	...	...	(one of 4oz., one of 5oz., four of 6oz., one of 7oz.)	15	10·40	3¾ to 24

		Average size.
Weights given in 12 cases s. c. fever, enlarged in	11 ...	16·08oz.
,, ,, ,, 43 ,, rem. ,, ,,	,, 39 ...	16·60 ,,
,, ,, ,, 18 ,, enteric ,, ,,	,, 15 ...	13·50 ,,
,, ,, ,, 7 ,, cholera ,,	none...	5·71 ,,

Weights of spleen actually given in 69 *out of the* 92 *cases.*—Four cases, returned as "natural," have been put down each as 7oz.; thus making, in all, 73 cases of which the weights are held as given, leaving 19 of which the weights have not been recorded. Spleens weighing 8oz. have been held to be enlarged.

As regards the influence which age, and period of residence in India, exert in determining the occurrence of severe forms of fever, the lesson to be drawn from the history of the cases under review bears out what experience had already given reason to infer, namely, that fever is more apt to be attended with prominent cerebral symptoms, and for that reason to be returned under the heads of remittent or continued fevers, in the older soldiers, and that, too, in the earlier period of their residence in the country, than happens with the younger men.

In the 49 cases set down as remittent fever, the average ages were over 26, whilst the average period of residence was under 4 years.

In the 13 cases shown as deaths from simple continued fever, this is especially remarkable in the matter of the period of residence, the average of which was a little over $1\frac{1}{2}$ years, whilst the average age was a fraction over 25.

As to the 30 cases returned under the head of enteric fever, and so named, in the majority of instances, on account of the marked lesions of the intestinal glands discovered after death, the view now held as to the especial liability of the young to suffer from those lesions in a more than usually marked degree, was likewise borne out by the facts collected, as will be seen on referring to table No. 6, which is subjoined.

Table No. 6.

Showing the average ages and periods of service in India, together with the duration of the disease in 92 fatal cases of fever.

Form of Fever.	Total of cases.	Ages.		Periods of service in India.		Duration of disease.	
		Average years.	Range.	Average years.	Range.	Average in days.	Range.
Simple continued fever ...	13	25·03	19 to 47	1·55	$\frac{5}{12}$ to $3\frac{2}{12}$	12·53	4 to 38
Remittent fever ...	49	26·69	18 to 43	3·68	$\frac{1}{12}$ to $21\frac{6}{12}$	13·55	2 to 70
Enteric ,, ...	30	23·28	$18\frac{9}{12}$ to $30\frac{6}{12}$	2·24	$\frac{1}{12}$ to $11\frac{10}{12}$	15·80	4 to 62

Tables Nos. 7 and 8 which follow, show the months over which the fatal fever cases were distributed, and the information they supply corroborates what has been said in a previous part of this paper as to the general distribution of particular forms of fever over the months of the year—No. 7 showing this in regard to 90 cases (inadvertently made 92 in the other tables in the way already explained) occurring in 1871, of which special details have been given above; and No. 8 giving the total cases of fever that proved fatal between the 26th March 1871 and the 25th March 1872: comparative statements of the fatal cases of cholera, and of sunstroke and heat apoplexy, occurring within that period, having, in this last instance, been added for future reference.

TABLE No. 7.

Showing the months of 1871 *in which the* 90 *deaths from fever occurred.*

FORM OF FEVER.	MONTHS OF PREVALENCE.										TOTALS.
	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	
Simple continued fever ...	0	2	2	1	2	0	4	1	1	0	13
Remittent fever ...	0	3	4	6	5	8	9	5	5	4	49
Enteric „ ...	0	0	1	5	6	7	3	3	2	1	28
Totals ...	0	5	7	12	13	15	16	9	8	5	90

TABLE No. 8.

Showing the months in which the total deaths from fevers, cholera, and sunstroke or heat apoplexy, occurred among British troops in the Bengal command, between 26th March 1871 and 25th March 1872.

DISEASES.	From March 26th.	April.	May.	June.	July.	August.	September.	October.	November.	December.	January.	February.	To March 25th.	TOTALS.
Simple continued fever ...	...	5	2	1	2	...	5	...	1	1	...	...	...	17
Intermittent fever ...	...	...	...	...	...	...	...	...	...	1	1	...	...	2
Remittent „ ...	...	9	6	8	7	9	11	6	5	4	1	3	...	69
Enteric „ ...	1	...	1	5	7	8	4	4	4	3	4	1	...	42
Total of fevers ...	1	14	9	14	16	17	20	10	10	9	6	4	...	130
Cholera ...	...	3	1	2	1	2	2	...	11	1	...	...	1	24
Sunstroke or heat apoplexy	...	1	1	9	1	3	3	...	...	...	...	...	...	18

From the nature of the evidence adduced above, the line of argument which the writer intends to follow out in his treatment of the inquiry he has taken in hand, will already have been inferred.

His chief aim will be, not only to show that all the non-contagious forms of fever above referred to are virtually one and the same disease, modified into apparent varieties by the influence of various attending conditions, but also to demonstrate that in no instance do those diseases owe their origin to the reception into the body of any specific germ (using this term in the sense employed at the present time with reference to the supposed origin of cholera and typhoid fever); but that in every case this is due, in the first instance, (returning to the text borrowed from the principles laid down by Sir Robert Christison,) to the influence exercised by that primary functional injury of the great cerebro-spinal nervous centres which forms the first appreciable link in the chain of sequences constituting fever, and concerning the invariable existence of which there can remain no manner of doubt on the minds of those who have closely studied the symptoms of fever.

He will endeavour, further, to show how strong grounds there are for concluding that all the

symptoms and pathological changes noticed in non-contagious fevers, are originated by this condition of the great nerve centres becoming the means of inducing important alterations in the ordinary vital actions of various parts of the human organism; such, for example, as may be brought about, in the case of the internal organs of organic life, through the "inhibitory" power which the presence of this condition may exert over the functions of the great ganglionic system of nerves within the abdomen, and, through the vaso-motor nerves which that furnishes to them, on the blood-vessels of the secreting surface and glands of the intestinal canal, as well as on that of the large secreting organs from the liver downwards; such, also, as may occur in regard to the surface of the body and extremities, and is exemplified in the action of the skin caused by the condition in question operating, probably, as will be explained further on, in a more directly exciting way (as that is understood in contradistinction to the opposite mode of nerve-action known as "inhibition") on the vaso-motor nerves of those particular parts; and, as may further show itself, in respect of the body generally, in the augmentation or reduction of its temperature,—both of which states there is reason to believe that the two opposite conditions of nerve-action above alluded to, once

set up, are capable of inducing, simultaneously, in different parts of the body.

Explained by the light of recent physiological researches into the nature of the action of nerve-force, it can be readily seen that once this functional injury of the great nerve centres has been produced, the influence which that condition is known to exert is quite sufficient to account for the initiation of all the phenomena of non-contagious fever, however much those may vary in form and type; and, when the matter is viewed in this way, the sequence of the phenomena of fever can be both readily traced and rationally explained.

But, when a more complete understanding of the causes of this initiating condition itself is sought for, we are compelled, from want of direct evidence, to attempt to fill up the voids in the continuity of our observed facts by calling in the aid of considerations of a more speculative character.

Much as this position of matters is to be regretted, it ought still to be borne in mind, that, when speculation is founded on facts actually observed in connexion with vital phenomena, although those may have only an indirect bearing on the subject under investigation, there are yet logically sound grounds for hoping that the

progress of medical knowledge may be materially advanced by this mode of employing our reasoning faculties. It is only when speculation sets out from so-called fundamental truths, which, on examination, are found destitute of any basis either of actual facts or of sound logical induction, that discredit is brought on medical speculation, and the progress of medical knowledge is so painfully retarded.

Whilst, therefore, accepting all that has already been advanced as to the nature of the remote causes of non-contagious fever considered from the "malarial" point of view of their origin, and admitting that, of all the many influences which may induce this disease, those of a malarial nature are perhaps the most powerful, and most universally distributed, it yet seems desirable, at the present day, to attempt to advance somewhat beyond the limits of the old groove in which the study of this problem has so long moved, and to consider whether, in addition to influences which would appear to act—reasoning by analogy from the known facility of action of medicinal substances applied by inhalation, and from the effects caused by breathing the vitiated air of overcrowded and badly ventilated dwellings,—by inducing some change in the blood which they reach by the air passages and lungs, or which, to some extent, they

may affect by absorption from the alimentary canal, in the case of food or drink having become impregnated with them, there may not also exist others, operating more directly through the nervous system alone, which may be entitled to claim a considerable share in the production of non-contagious fever.

The evidence available on this point would go far to make it probable that some influences of a purely impressional character, acting through the mind, and others of the nature of sensations conveyed from the surface to the cerebro-spinal centres by afferent nerves connected with either general or special sensation, may also play a very important part in bringing about the initial functional injury of the great nerve centres, to which all the subsequent changes connected with the form of fever now being considered must be traced back. The effects of the emotion of terror in not only relaxing involuntary muscles (in the same way as happens to the coats of arteries by the "inhibition" of the action of sympathetic nerves supplying them), but in actually producing sudden watery discharges from the intestinal surface, and the influence of mental anxiety acting in the same direction, are well known.

Exhaustion, also, from over-exertion; the feelings of distress produced by exposure to sudden

or great cold on the one hand, or to prolonged heat on the other; the painful impressions conveyed through the optic nerve from the action of bright light long endured, as well as those caused by the action on the olfactory nerves of intensely disagreeable and persistent odours (in addition to the effects produced by actual absorption of the particles which give rise to them), may all, likewise, be noted as potent agents capable, it seems rational to believe, each in its own way, of contributing to that over-stimulation of the cerebro-spinal nerve centres which eventually leads to their functional derangement.

Again, there are subtle but most powerful influences from the effects of which the human body can never escape—those to which nerve-action itself bears so close an analogy, and which are constantly present in ever-varying intensity—namely, electro-magnetic conditions of the earth and atmosphere, the effects of which on the human organism, the writer cannot but think, have never yet obtained that consideration as means of influ encing health which their importance deserves.

He believes, moreover, that if the degree and mode of electro-magnetic action in this respect, were made the subject of systematic investigation, the full bearings, not only of "pandemic-wave" influences, but also,—considered in the light of the

more recent electro-magnetic discoveries,—of that exerted by the presence or absence of sun-spots, and, probably, of other phenomena connected with changes observed on the sun's photosphere, on the prevalence of epidemic diseases of the nature of those now under consideration, might come to be considered worthy of a higher place in relation to the explanation of the production of disease, than has as yet been generally accorded to either of the theories lately advanced in connection with those subjects.

Whatsoever the origin of the condition of the central nervous system, already so often referred to, may be,—and there are strong reasons for concluding that many and widely differing causes may contribute to it,—the writer would wish to impress distinctly on the minds of his readers his firm belief, which he hopes to be able to show is founded on observed facts, that the immediate influence which it exercises is not of a "specific" nature, in the sense of its being capable of producing one form of disease, such as enteric fever, and that only; but that its action is such as, when it has attained sufficient tension of force, probably through a process of "cumulation," to induce, eventually, a simple condition of change in the ordinary physiological action of certain vital processes within the body which leads to some

one of the many varieties of non-contagious fever.

Accordingly, on the explanation which the sequence of events shown by post-mortem examinations to be invariably consequent on the initiation of this description of febrile disease, it has become demonstrable that the altered vital action so set up—although at first consistent with health to a degree that renders it impossible to say where health ends and disease begins—may, simply by its duration being sufficiently prolonged, become developed, as stated above, under different co-existing states of the body, and varying external influences, into some one of the many forms and types of non-contagious febrile disease,—the intensity of the original exciting cause, and the nature of those modifying influences, determining whether the form of attack shall prove to be the milder ague at one end of the scale, or the highest expression of form in which disease of this description ever declares itself, at the other; or whether it shall be some variety holding a position intermediate to those two extremes.

In order, however, to attempt to explain still more closely what he means by the principles which he has, in a general way, just enunciated, the writer would now endeavour to show what amount of light the latest physiological views on

the subject of the influence exercised by the nervous system generally on the processes of circulation, nutrition, and secretion, and on the production of animal heat, have thrown on the nature and origin of the pathological conditions which so uniformly attend non-contagious fevers.

Certain clearly established physiological points, now about to be detailed, are those which most closely concern the present question; and the writer hopes to be able to show in the sequel, that, when taken together, and an effort made to connect them with the symptoms and pathological conditions found in fever, they afford a clue to the explanation of many of its phenomena which have been hitherto but dimly understood.

He is, moreover, even sanguine that their full application to the question under consideration, will prove a standing-point from which it may be possible to rescue our knowledge of the nature and means of prevention and treatment of the grand opprobrium of medicine, cholera, from the dominion of baseless theory and empiricism, under which all really scientific efforts to place its relations in those respects on a more rational footing, have hitherto been so completely crushed.

The following are the more important physiological considerations bearing on this inquiry to

which reference has just been made; and the writer will try to record them as briefly as possible, and in such a manner as may, perhaps, render unnecessary more than a simple reference to their different headings, in any subsequent endeavour through their means to explain the various phenomena of fever:—

1. The first point of importance to which allusion will be made is that which relates to the nature and action of the coats of the blood-vessels, and the influence which, under certain conditions, they are capable of exerting on the circulation, and, consequently, on the vital processes of nutrition and secretion in various important parts and organs of the body.

The middle or fibrous coat of arteries is perhaps the one which most concerns the present investigation, and to that, therefore, attention will now be altogether confined.

This middle arterial coat is made up of two layers—an outer, composed of yellow elastic tissue; and an inner, consisting of the smooth, unstriped muscular-fibre-cells of organic life.

Among the functions which the first-named layer exercises, is that of maintaining the equilibrium of the blood-ducts,—opposing their excessive contraction under strong action of their muscular-fibre-cells, on the one hand, or unusual

dilatation under circumstances of loss of power or diminished action of the latter, on the other.

This yellow elastic layer shows its highest degree of development in vessels of the largest size. It is however wanting in none, although it is found that, as a rule, it exists in inverse proportion to the degree of development of the muscular part—that being most marked in the blood-vessels of smaller size.

Its mode of action would seem to be of a purely passive and mechanical nature, dependent on the peculiar structure and properties of yellow elastic tissue.

The muscular portion of the fibrous coat of the arterial system, on the other hand, fulfils its functions entirely under the control of the vaso-motor nerves with which it is supplied,—it having been found, experimentally (speaking in a general way of the control exercised by those nerves), that their division or paralysis induces dilatation of, with increased flow of blood through, the arteries to which they are distributed; whilst their stimulation by galvanism or other means, applied where no ganglion intervenes between the point at which the stimulus is made to act, and the vessel which the nerves supply, produces a degree of contraction and of diminished blood-flow proportioned to the intensity of the exciting force employed.

To such a degree may this last be carried, that the calibre of the vessel acted upon by a strong galvanic current has even appeared to have, for a time, undergone complete obliteration.

2. In addition, however, to the general fact just stated, that stimulation of a vaso-motor nerve causes contraction, and division or paralysis, dilatation of blood-vessels, the experiments of M. Claude Bernard have demonstrated the existence of another and still more important form of nerve-action belonging exclusively to the case of a sympathetic vaso-motor nerve stimulated through its ganglion and the cerebro-spinal nerve branch communicating with the latter, but which cannot be distinguished, in its results, from that condition of similar nerves which follows their division or paralysis.

The vaso-motor nerve itself, thus acted upon, differs in no respect from any other in which direct stimulation leads to contraction of the blood-vessels it supplies—only, in the one case, the exciting force is applied to the nerve in a part of its course beyond a ganglion, whilst in the other, the stimulus is made to act through the communicating cerebro-spinal nerve and the sympathetic ganglion with which that is in connexion.

To this third description of vaso-motor control the term "inhibition" has been applied, to

distinguish it from that similar condition of a vaso-motor nerve which is caused by its division or paralysis.

The fact of M. Bernard's experiment having been made on the nerve-supply of an outlying organ intimately related, both in function and structure, to other organs of the great digestive and assimilative track with which our present inquiry is so closely connected, and of which the sub-maxillary gland (the one experimented upon) really forms an accessory part, renders the results obtained by that observer of very great value as a means of showing what form of action would, in all probability, be produced, were a similar exciting force applied, within the chest and abdomen, to the whole or portions of the great sympathetic system from which the nerve-supply of the intestinal canal, and of the abdominal glandular structures generally, is derived.

That the action so excited would be identical, and the results of "inhibition," in regard to the abdominal structures and glands, the same as are witnessed in that of the sub-maxillary gland, can scarcely be doubted when the existence of a like ganglionic arrangement on the nerves of those parts, and the fact of their communicating after a similar fashion with the cerebro-spinal system, are duly considered.

Should, however, any doubt exist on this point, it can hardly fail to be at once dispelled by the knowledge that, in experimenting on the lower animals, Professor Moreau has, by section of the sympathetic within the abdomen, not only caused a condition of the blood-vessels of the parts within that cavity identical with what follows division of the sympathetic elsewhere (as, for instance, in the case of the sub-maxillary gland itself), in regard to their dilatation, the increased rapidity of circulation within them, and the augmented functional activity of the part to which that leads, but has been able, further, to demonstrate that one result of such section is the production of a watery flux from the intestinal surface, differing in no respect, either in appearance or in chemical composition, from that seen in cholera in the human subject.

3. Our anatomical knowledge of the distribution of the sympathetic system of nerves in the neck, chest, and abdomen, is now very complete; and our physiological acquaintance with its relations to the glands and special structures of those parts, has been so much supplemented by the results of Bernard's and Moreau's experiments, as to enable us to say that the vaso-motor nerve-supply of those structures, so intimately connected with the functions of purely organic life over

which the will has no control, is derived almost entirely from this peculiar system of sympathetic nerves.

It has, till lately, been held that the vaso-motor nerves of every part of the body belong exclusively to the sympathetic system. The recent observations of Schiff, however, have shown reason to believe that the blood-vessels of the skin, and of the trunk and extremities generally, have a nerve-supply of a character which is, as regards its functions, in a great measure identical with that displayed by the cerebro-spinal motor system.

It would appear, therefore, from this, as if nature had provided a system of regulating-sympathetic-nerves for all parts within the body connected with its purely organic functions over which the will has no control; but that, in supplying a different system of cerebro-spinal motor nerves for the regulation of the voluntary action of the trunk and extremities, it had either altogether utilized those motor nerves for carrying on and regulating (in addition to their own special functions connected with voluntary processes) the nutrition of parts, such as the skin, &c., which are accessory to the bodily structures and amenable to the will, or had there so associated the cerebro-spinal motor nerves with

the sympathetic ones, by inter-communication of fibres—and that, too, at a distance from the regulating ganglia of the latter—as to make it inevitable that any unusually powerful influence proceeding from the central cerebro-spinal nervous system, and acting beyond the control of the will, should likewise impart to the sympathetic vaso-motor nerves a directly exciting influence, similar to that which the cerebro-spinal nerves exercise, and which would act without the intervention of the "inhibitory" process, which, from the anatomical nature of things existing there, must result, within the chest and abdomen, when a similar influence exerts itself on the great sympathetic system in those regions of the body.

The general result of the existence of this double vaso-motor system, within and without the body, must consequently be, that any common influence of a purely efferent character proceeding from the great central cerebro-spinal nervous system, whatsoever the cause that may have excited it there, will initiate, simultaneously, two diametrically opposite conditions, in the parts connected with purely organic life within the interior of the body, and in the external parts more closely connected with those bodily processes over which the will can exercise control,—producing, in the first, the inhibitory nerve-action

leading to dilatation of blood-vessels, with increased nutrition, secretion, &c., and a rise of temperature ; and, in the last, exciting the opposite conditions of contracted blood-vessels, shrunken tissues, and diminished temperature.

Further, the above considerations taken, as a whole, in connexion with Bernard's and Schiff's observations, and viewed from the basis of Sir Robert Christison's principles as to the nature of the initiating process in fever as that manifests itself at the great cerebro-spinal centres, not only rationally explain the sequence of all the initial phenomena of fever—and consequently, as will be shown further on, of cholera also—but even supply corroborative testimony as to the accuracy of the views of the physiologists named above, inasmuch as the pathological results seen in the condition of the blood-vessels of internal organs after death, in non-contagious fevers, are exactly such as can only be explained, in a way that shall not do violence to nature, on the supposition that the interpretation which the above-named observers have put upon the results of their experiments is the correct one.

4. Next to M. Bernard's discoveries as to the degree of influence, and the mode in which that is exerted, possessed by the nervous system over the functions of secretion, the knowledge which

we now enjoy as to the relations that exist in the pyrexial state between increased ureal elimination, a rise in the temperature of the body, and the condition of the circulation in the kidneys which leads up to those phenomena, is perhaps the most important in its bearings on the subject now under consideration.

For the present extent of our knowledge on this point we are mainly indebted to Professor Parkes, by whose researches it has been placed on a clear and satisfactory basis.

Dr. Parkes has shown, very conclusively, that the amount of nitrogen excreted from the body in the forms of urea and uric acid, in health, as nearly as possible balances the quantity of nitrogenous materials taken into the system; that, so long as the body continues in a state of health, neither unusual muscular exertion nor great mental work, neither atmospheric conditions nor any other influences, have the slightest effect in disturbing the relations that exist between the amounts of entrance and exit of this element. The moment, however, that disease sets in, the relations in question are altogether set aside, the amount excreted becoming then out of all proportion greater—in one instance mentioned by Professor Parkes, nearly thirty times more—than that taken into the body.

Apart from their general physiological value, the existence of the laws established by Professor Parkes's observations is of special import in connexion with the present inquiry, inasmuch as the knowledge we now possess as to all that this increased ureal elimination implies, enables us to speak with an amount of precision as to the condition of certain internal and unseen parts which would otherwise have been unattainable, and concerning which it is of the first importance that we should be informed.

Professor Parkes's researches have, moreover, been supplemented, in a very important manner, by the discovery, made by Dr. Sidney Ringer, that the disturbance of the relations ordinarily existing between the amount of nitrogenous materials taken into and excreted from the body consequent on diseased action, begins, in ague, from twelve to twenty-four hours in advance of the initial rigor hitherto held to have been the actual commencement of that disease.

It is of importance, also, that it should be here noted that Drs. Adams and Walsh, of the British Medical Service, recently, and a Russian physician, at an earlier date, arrived at the conclusion, from observations made on the state of the general circulation alone, that in cholera, just as we have seen happens in ague, there is a period of

departure from the usual conditions of health, of like duration, which precedes the outset of the characteristic purging and collapse of that disease.

The writer would here express his conviction, that, when duly searched for, increased elimination of urea will be found to mark this preliminary stage of cholera also, just as it does the corresponding one of ague.

It should further be observed, however, in connexion with the preceding remarks, that it has long been known, that, for from twelve to twenty-four hours previous to the occurrence of rigor in ague, there are generally present some degree of malaise, of pain in the back or limbs, and often, too, of increased frequency of micturition. The symptom last named was specially noted by the older writers; and it is of importance to bear in mind the fact of its recognition as one of the premonitory signs of ague, because considerable weight has rather recently been attached to it as a precursor of heat apoplexy.

The writer will take occasion, further on, to again allude to this point.

5. It now only remains to notice—chiefly as a means of, in some measure, explaining the periodicity which, in one form or another, most non-contagious febrile diseases display—the observation made many years ago by Dr. Cullen,

to the effect that the exciting influences conveyed from the nervous centres, in processes connected with the maintenance of life which are but little under the control of the will, are invariably of an intermittent or wave-like character.

We know from actual observations made in the case of severe wounds of the abdominal walls, and from what has been seen during certain surgical operations, that the action of the involuntary muscles of the intestines is quite in accordance with this view of the matter—being made up of periods of alternate contraction and relaxation following each other at short and tolerably regular intervals.

The rythmical movements of the heart and lungs are examples of the same form of action; and, recently, MM. Legros and Onimus have shown that waves of contraction and relaxation, precisely similar to those noticed in the intestines, occur also in the walls of the smaller blood-vessels.

All physiological observations and experiments, moreover, tend to demonstrate the existence of a law by which every act of exercise of an influence leading to contraction of involuntary muscular fibre, on the one hand, or to inhibition of such action, on the other, in whatever structure that may occur, is followed by a corresponding period

of suspension of whichever of those actions may have been set up, and of the temporary establishment of a condition of an opposite nature.

It would appear, so far as our knowledge of them extends, that, in health, the rule in regard to those alternating actions is, that they are brief in duration, and succeed each other at short and regular intervals.

When, however, anything has occurred to overstrain, or functionally to injure or disarrange, the action of the great cerebro-spinal centres whence those influences proceed, there is, in the results which those deranged actions lead to, sufficient evidence to show that the impulses which, under the ordinary circumstances of health, originate the alternating conditions mentioned above, become unnaturally violent. Their effects become, in consequence, so prolonged, as, bit by bit, to lead to a subversion of the natural vital action of the organs and tissues influenced by this change, simply as a result of the undue prolongation, as that affects the muscular coats of their blood-vessels, of each act of those two opposite processes of contraction and relaxation.

It is only by a due consideration of all these points, in relation to the symptoms observed and the nature of the tissue-changes seen in structures in which this abnormal action has been going on,

that we are enabled to form some conception of the mode of production of periodicity, and the degree of persistence of that condition which, it may be observed, is, as a rule, maintained in non-contagious fevers, so long as the more natural action of the nerve-centres has not been completely thrown out of gear by the positive diseased state of those parts which their earlier functional derangement is always tending to set up. The longer this condition of departure from health lasts, the more it will serve, by the consequences to which it leads, to reduce diseased functional action, from a state of periodicity, to one that becomes gradually continuous, prolonged, and less and less capable either of displaying any degree of the special characteristic above referred to, or of manifesting the quality of intermission in any form.

The condition of periodicity which persists, in various degrees, up to a certain point in all the forms of fever of the class now under consideration, must, therefore, be held to be merely the modified remnants of certain ordinary vital processes essential to health. The gradual departure from those normal states of action witnessed at the outset of fever, is, probably—judging from the lesson to be derived from a careful study of the phenomena of ague—at first shown by

prolongation of the waves of action and interval of which this natural periodicity is made up, as a consequence of abnormally increased intensity of the influence which excites them. Ultimately, however, this change would appear to manifest itself, in its more advanced form, by the entire disappearance of all trace of those distinctive processes, and the substitution for them of continued diseased action.

The writer will now close his enumeration of physiological facts bearing on this investigation, by noting that an association of cerebro-spinal nerves with those of the sympathetic system, exists, in the case of the nerve-supply of the heart and lungs, similar in principle to that which holds good in relation to the abdominal organs.

The nerve-arrangements which immediately concern the heart's action, would appear, however, to be of a more elaborate description than those provided for the abdominal organs; experiment having shown strong reason to infer, in regard to the former, that influences proceeding from the great nerve-centres may act upon the heart through two different channels, and with results which are nearly opposite in character,—those reaching it through the lower cervical and upper dorsal sympathetic ganglia, causing accelerated action with diminished force; those arriving

at it through the pneumo-gastric nerve, producing, on the contrary, a retarding or depressing effect.

As regards the nerve-arrangements of the lungs, there is, on the other hand, nothing either to show that the vaso-motor nerves of the pulmonary capillaries are different in action from those of the abdominal organs, or to give reason to infer that results differing from those which occur in the latter, would follow the action of any inhibitory influence coming to them from the cerebro-spinal nerve-centres, and operating through the sympathetic ganglia and the nerves proceeding from those.

So far as can be seen, therefore, there is every reason to believe that neither the action of the heart, nor the circulation within the lungs, can possibly escape answering—each in its own way, and in accordance with what has been stated above—any general influence exerted by unnatural conditions arising within the great cerebro-spinal nerve-centres; and that, too, not in a manner secondary to changes already set up in the general tissues of the body, and acting through the influence of an alteration in the condition of the blood affecting the heart and lungs directly, but primarily,—as happens simultaneously to the minute blood-vessels of other parts of the body,—as the direct result of that exciting nerve-influence alone.

CHAPTER III.

EXPLANATORY AND DESCRIPTIVE.

HAVING, in the preceding chapter, given a series of general details regarding the leading symptoms, and more prominent pathological appearances, witnessed in a fair average series of well-observed cases of fatal non-contagious fevers; and having also enumerated, in a general way, certain physiological considerations likely to assist the investigation of their intimate nature, the writer would now endeavour, more closely, to explain the phenomena of those fevers in association with their symptoms and post-mortem appearances, and in relation to the bearings which the whole of the data he has accumulated may have on their elucidation.

Before, however, proceeding to treat of the various phenomena of non-contagious fever in the way above indicated, the writer would at once state that, when speaking of ague as the type of this description of fever in its uncomplicated form, he wishes it to be distinctly understood that he likewise includes under this term those attacks of

fever which are so constantly witnessed in soldiers serving in India, and which are, by a large proportion of army medical officers, returned under the heads either of "febricula," or of "simple continued fever."

The writer's own experience in the medical charge of troops, has long convinced him that the attacks usually so returned are simply examples of ague in a slight form, which, either from the original mildness of their exciting cause, or from prompt treatment, or timely rest and nursing, have come to an end at the close of the first paroxysm (having been, in fact, cases of aborted ague), instead of passing on to regular intermittent fever; or, it may be, advancing, through the remittent into a form of a still more continued character.

The short, mild, description of fever in question, may, however, be but too readily aggravated into any of the more advanced degrees of the disease, by imprudence, by inappropriate treatment, or by neglect; and, to the writer's mind, no question of doubt can exist as to the actual identity of the whole series.

The evidence adduced in an earlier part of this essay, as to the frequent changes of nomenclature found necessary in the course of the progress of cases of fever, and as to the desirability

that this should have been carried to a still further extent,—which the recorded post-mortem evidence clearly shows to have existed,—in some measure, also, bears out the truth of what has just been averred.

Of the degree in which the initial condition of derangement of the great cerebro-spinal centres, already so often referred to, exists, we have no means of arriving at any estimate prior to the occurrence of those changes marked by various indications connected with the nervous and circulatory systems, and with the secretion of urine—more especially as regards the increased elimination of urea and uric acid which accompanies this latter.

That some appreciable degree of change of function of the great nerve-centres must exist in advance of the development of any of the manifestations just mentioned, it seems reasonable to infer. At present, however, we know of no means by which the extent to which this occurs can be measured.

We can now, however, with tolerable confidence predict, that, once the existence of the preliminary stage has become fairly evident through the presence of some one or all of the conditions above indicated, the initial rigor will probably follow within twenty-four hours of that occurrence.

This symptom will vary in duration according to the severity of the attack—both the intensity and duration of the rigor bearing a direct proportion to each other, and to the degree of the original exciting cause. It may, therefore, range from its mildest appreciable manifestation, to that highest degree of expression it is capable of displaying as witnessed in the state of collapse which forms the principal feature of cholera.

Evidence has already been adduced to show that there are strong grounds for believing, that, when the rigor has been fairly established, there are going on, simultaneously, in different parts of the body, conditions of circulation, secretion, nutrition, &c., which are exactly the opposite of each other—namely, of inhibition of nerve-action, in all parts within the body supplied by the nerves of the great sympathetic system, leading to dilated blood-vessels, activity of circulation, and all the conditions of increased nutrition, secretion, &c., consequent on that; and, of the opposite state, at the surface of the body and extremities, of direct increased nerve-action, leading to contracted blood-vessels, diminished flow of blood, shrunken tissues, and reduced temperature.

Before proceeding further, however, it seems desirable to notice here a consideration of much practical importance in regard to its bearings on

the explanation of certain ambiguous states of health which have, hitherto, been difficult to account for; and in respect, also, of the light it throws on the mode of origin, not only of some forms of non-contagious fever, but likewise of some descriptions of disease which have been always reckoned as being in a measure allied to that complaint. This is connected with the stage of the disease already referred to as occurring prior to the first onset of rigor; and it consists in this, that the very existence of the various conditions which mark the presence of the initial stage in question, goes to show that the sympathetic nerve-system which regulates all the important functions essential to animal life, must respond to the influences exerted on it by the great cerebro-spinal nervous system, with a degree, so to speak, of hair-trigger-like sensitiveness which is altogether wanting in the more direct action that seems to belong to the apparently more purely spinal nerves regulating the circulation, nutrition, &c., at the surface and extremities of the body—it being evident from the results observed in the latter situations, that the exercise of the full cumulative force of the initial cerebro-spinal functional injury, is required to bring into play, through the nerves supplied to them, the condition of action of the blood-vessels at the surface of the body and

extremities witnessed there during the persistence of the rigor—the action of inhibition spoken of above as having been already set up within the body previous to the occurrence of the latter symptom, undergoing, also, a simultaneous and proportionate increase.

Believing, as we have the strongest reason for doing, that the two opposite conditions of the circulation, produced simultaneously within and at the surface of the body, have one common origin, it is only reasonable to suppose that on this exciting force ceasing to act violently, there will be a corresponding abatement of both the sets of conditions above referred to; and that the cessation of the rigor will indicate the time when inhibition within the body will also become less marked.

Our knowledge, however, of the phenomena of fever, and of the nature of the sequences which occur in conditions of action of the blood-vessels and circulation excited through the nervous system, teaches us that a stage of reaction must follow on any degree of constriction of vessels and retardation of the flow of blood through them consequent on unusually prolonged contraction of their muscular coats. The sweating stage of ague must, therefore, on this view of the matter, necessarily follow, as a natural

consequence, on the opposite state of the circulation indicated by the presence of the conditions which constitute rigor.

That the action of inhibition within the body follows a law similar to that which, at the surface and extremities, leads to the change from the state of rigor to that of reaction, there is every reason to believe; but, as we are aware that, in the usual progress of ague, a second paroxysm similar to the first will, in all probability, follow it within a certain number of hours, we know that the condition of the central cerebro-spinal nervous system which led to the initial outburst, must still exist; and, further, that the minor degree of inhibitory influence which preceded the first attack of rigor will, in the ordinary course of things, be found to exist in advance of the second also. In fact, there is evidence to show, in the increased elimination of urea going on during life, and in the signs of increased functional activity and congestion of vessels of all the internal organs and tissues which remain after death—not only as this is witnessed in cases of fever which have been of ordinary duration, but as is seen, also, in a still more marked degree, in the examinations made in an earlier stage of the disease in deaths from sunstroke and cholera (both of which, the writer hopes to adduce conclusive evidence to show,

are merely forms of non-contagious fever)—that some degree of this inhibitory process is persistent throughout, in ague, to an extent that leaves no true interval of health in the periods which separate its paroxysms, however purely negative as to evidence of this, the results connected with the manifestations of rigor and reaction at the surface of the body may prove to be.

From what has already been recorded on this part of the subject, it will be seen that the influence which leads to non-contagious fever may manifest its presence in every degree of shade of expression.

It has, moreover, been further shown that we have good reason for believing that ague may exist without advancing beyond the display of more than one paroxysm.

Is there, therefore, any reason why, under a still milder form of the exciting cause of the complaint, the disease should pass at all beyond the stage of mild inhibitory action which has been mentioned as preceding the initial rigor?

The whole of the evidence connected with this subject, on the contrary, bears out the probability of such an occurrence; and the writer, further, firmly believes that in the actual existence of such a condition will be found to lie the explanation of those states of low health, hitherto attributed

—and, so far, rightly so—to malarious influences, which end in liver complaint, in dysentery, or, not unfrequently, in insidious attacks of fever showing bowel lesions in their most marked forms,—the occurrence of all those conditions being consequent on the state of undue activity of the ordinary vital processes in organs, kept up by the influence exerted on the splanchnic nerves by a minor degree of functional injury of the cerebro-spinal nervous system ; and which eventually leads to disease being set up in any internal organ or tissue which may happen, at the time, to be what is commonly termed "a weak point"—whether that may have reference to the liver, or to the large intestines, in the event of the disease proving to be hepatitis, or dysentery, in the older men; or to the intestinal glands, should the resulting disease prove to be fever occurring insidiously in subjects of a less mature age.

The following testimony, which will, in some measure, assist in showing that the views just expressed are not altogether without foundation, and which would appear to have forced itself upon an independent observer in the course of his practical experience in the treatment of tropical diseases as they attack European soldiers in India, will come in opportunely at this point.

It occurs in the annual report of the 85th

Light Infantry stationed at Meean Meer in 1869, and is to be found in the remarks by Dr. Skeen, surgeon of that corps, which are quoted below :—

"The attacks of diarrhœa, dysentery, hepatitis, jaundice, and congestion of the spleen, were, in most instances, connected with or the sequelæ of ague ; their treatment at head-quarters was conducted on this assumption with satisfactory results."

If this explanation should prove to be applicable to what happens in regard to the above complaints as they are met with within the tropics, the writer believes that it will be found to be doubly so in temperate climates, in regard to those insidious forms of ill-defined loss of health which so frequently occur there, on which so-called enteric fever so often supervenes, and which, it is reasonable to believe, may have been excited by the milder degree of malarial injury likely to be met with in countries where those causes of disease cannot fail to be, on the whole, less virulent than they are in the tropics.

Having completed this important digression, the writer will now resume the further consideration of the phenomena of fever.

The preliminary period of departure from health ; the cold stage, and that of reaction which succeeds it ; the still further presence of a preliminary stage, similar to the first, which shows

itself prior to the subsequent rigors of succeeding paroxysms; the recurrence of those paroxysms; the early death by the sudden supervention of coma, or the slower fatal termination by the gradual setting in of a more continued form of fever attended with delirium, and likewise eventually ending in coma and death; the gradual appearance of enteric symptoms, attended or not by an eruption, and proving fatal (later in the disease than is, as a rule, seen to happen in any of its other forms) by asthenia, or by a combination of that with coma,—these are the leading symptoms and modes of termination which, in fever, follow each other much in the order in which they have been set down above, but which manifest, also, in their sequences of occurrence, a certain amount of relationship to the period of duration of the disease, and to the fact of whether head or bowel complications may hold the most prominent position.

An attempt will now be made to explain the nature and origin of those changes within the abdomen and head which post-mortem examinations reveal, and to connect their presence with the symptoms observed during life.

And first as to those met with in the abdominal viscera.

Of these, the appearances observed in the intestinal mucous membrane, and in the glands

connected with that structure, are by far the most important.

Three conditions occur there with varying degrees of frequency; namely, congestion of the vessels; enlargement of the solitary and clustered glands alone; or enlargement of those structures, attended with either inflammation, ulceration, or sloughing, alone, or with all three of those latter processes at the same time.

Tabular statements showing the degrees of prevalence of those different conditions have already been given on an earlier page. From those it will be seen that, of the appearances in question, the existence of various degrees of congestion is the condition which is of most universal prevalence. There is, moreover, it would appear, no part of the intestinal canal, from the stomach to the large intestines inclusive, which may not present more or less of it.

The part, however, in which it was met with most frequently, and in by far the most marked degree, was, as might have been expected, the lower two or three feet of the ileum, where (for some reason in connexion with the human economy which we do not yet fully understand) the natural vascularity of the tissues, and the presence of intestinal glands, attain the highest degree of development to be met with in the intestinal track.

Whilst the evidence collected shows that congestion was of almost universal prevalence at every stage of the disease, it demonstrates also, very conclusively, that, in proportion to the duration of the complaint, this condition was supplemented by the two sets of changes mentioned above as occurring in connexion with the solitary and clustered glands—various degrees of their enlargement and prominence being spoken of in the casualty reports; in connexion with the solitary variety, as having resembled "duck-shot," "swan-shot," &c., as those conditions were witnessed at the earlier stages of the disease; whilst inflammation, ulceration, or sloughing of both sets are recorded as having occurred in the cases that proved fatal at its more advanced periods—notably so, in the most marked degree, in the younger men who fell victims to it.

There is evidence to show, further, that the increased prominence of the glands is simply the result of their distention consequent on an unusual development of their natural contents, caused by persistent hyper-activity of the circulation and nutrition in the parts, from inhibition of the vaso-motor nerves of their blood-vessels. This distention of the glands gradually increasing, from their augmented action being in excess of the requirements of their ordinary functions (whatever

those may be), comes, at length, to induce in them such a degree of tension of their textures as leads to their inflammation and subsequent destruction by ulceration or sloughing.

Why diseased action of so serious a nature as that just named should come to be so readily lit up in the intestinal glands from a cause apparently so trivial, will be more easily understood when the following anatomical peculiarity connected with those structures is taken into account. It consists in this, that whilst the mucous membrane of every other part of the intestines is so loosely connected to the subjacent parts as to admit of its being freely moved over them, and to allow of its undergoing much stretching, through distention of the bowel, without suffering injury, the portions of it which cover the intestinal glands are not only closely adherent to those bodies, but are so firmly attached at their circumference to the subjacent textures, as to altogether prevent any lateral motion, or to admit of but limited yielding of the parts over which they are stretched, and which they thus, in a measure, bind down. The consequence of this arrangement is, that, when over-distention of the glands takes place, owing to mere increase of ordinary nutritive action from that unusual activity of the circulation around them, which the inhibition of the vaso-motor

nerves, already so often mentioned, has set up, the unyielding nature of those parts—in simple obedience to a law well known in connexion with surgical pathology—re-acts unfavourably on the tissues concerned, and lights up in them inflammatory action, which, suffering aggravation from the constriction to which the parts immediately concerned are subjected, rapidly runs on to its higher results; of ulceration in the milder forms; and of sloughing in those of most aggravated degree.

Careful investigation of the pathological anatomy of the lesions now under consideration, has shown that the inflammatory process connected with their production is attended with some amount of its usual exudation, and that this takes place, to some extent, into the tissues immediately surrounding the affected parts.

Judging from the evidence supplied by the post-mortem records that furnish the data on which these remarks are founded, the mode of destruction of the glands does not appear to commence with any degree of uniformity even in neighbouring clusters. In some instances the first indication is spoken of as having been a small point of ulceration; in others, the glands are stated to have appeared as if "cracked across;" whilst, in a third form, a complete slough would seem to have formed at once.

When, however, whether by ulceration, or by sloughing, the destruction has been completed, and the contents of the gland have escaped, ulceration would still appear to have, as a rule, continued to make some advance into those adjacent tissues which have had their vitality weakened by their vicinity to, and some participation in, the neighbouring inflammatory process. On the degree to which this reduction of vitality, and the occurrence of inflammatory exudation, have taken place, would seem, in a great measure, also, to depend the distance to which the ulceration may extend; and, consequently, the amount of risk which is run of perforation of the bowel happening.

It is, of course, whilst this extension of the ulcerative process is going on, that the danger of serious hæmorrhage occurring from its laying open a blood-vessel of any considerable size, exists.

From the various considerations which have just been submitted in elucidation of the origin and progress of the intestinal lesions witnessed in fever, it will, the writer hopes, be admitted how little necessary it is, in seeking to explain the nature of those changes, to call in the aid of any theory which rests on the assumption of the existence of a specific poison manifesting its presence, either in the course of its elimination from the blood, or,

by acting more directly on its absorption from the intestinal canal.

The writer can only state, that he has altogether failed to discover that any real evidence exists which can possibly be made to support such a belief. He feels satisfied, however, that the explanation which he has just recorded, as it is based on observation of the chain of sequences which can be traced, link by link, from beginning to end of the process, will, eventually, be accepted as the only one which can be considered to be, in all respects, fairly true to nature.

The existence of congestion and enlargement of the mesenteric glands, and the presence of deposit within those structures, have been reckoned as evidence in support of the position which assumes that the destructive action, on which those lesions of glands depend, owes its initiation to a poisonous material of a specific nature. The writer would, however, explain this after a much more simple fashion.

To his mind, the presence of the congestion in question only suggests that the capillary circulation of the mesenteric glands, in common with that of every other texture within the abdomen provided with vaso-motor nerves from the same source, has not escaped the general condition which inhibition of the sympathetic nerve-action brings

about there; whilst, as regards any more advanced form of diseased action that may be set up in those glands coincident with the presence of well-declared intestinal lesions, that occurrence would appear to him only to afford conclusive proof that the mesenteric lymphatic system obeys the same general law which leads to a like affection of analogous structures in other parts of the body, as may be seen when even a simple boil shows itself on the skin in the vicinity of a lymphatic gland, or on the course of the lymphatic vessels leading to it, even in cases where no suspicion of the existence of "specific" action exists.

As to any information likely to throw light on this matter, which the symptoms specially connected with it afford, that would appear to be altogether of a negative character.

Why pain should be excited there, and gurgling be caused by pressure applied over the region of the right iliac fossa, is too self-apparent to call for any notice here; and, as for the lesson to be learned from the nature of the bowel discharges, it will at once occur to those who have had much experience of tropical diseases, that there is little to distinguish those attending fever showing enteric lesions from such as so constantly accompany advanced dysentery, when the acute stage

has passed and only open ulcerations remain—the causes of the peculiar nature of the discharges being the same in the ulcerative stage of the bowel lesions of fever, and in the more advanced period of dysenteric disease. The conditions also which may lead to accidental hæmorrhage, and to perforation of the bowel, which may occur in dysentery as in the bowel lesions of fever, are likewise similar, although, in the former, the frequency of those accidents is, on the whole, less marked than in the latter—the nature of the progress of events in the two diseases diverging somewhat at this point.

The writer has a few remarks to offer (and he thinks they may most conveniently be made now) on the subject of the so-called typhoid eruption so frequently observed to occur in cases of fever showing bowel lesions, and of which the presence has been held to be strong additional proof of that complaint being a blood disease allied to, if not in reality one of, the exanthematous order.

That, in the instances under consideration, the disease could not have been of an exanthematous nature, is proved by the fact already recorded above, that out of 92 fatal cases of which the histories and post-mortems were considered at an earlier part of this essay, only 10·87 per cent. showed what was called a true typhoid eruption—

the whole of that number, however, presenting decided intestinal lesions; whilst the total number in which the characteristic ulceration of the glands occurred in a marked degree, amounted to no less than 51·08 per cent.

The writer believes that this occasional symptom may, like several others that have already been touched upon, be rationally explained without the aid of the assumption that blood or other poisoning exists. And here, again, as in other instances already given, a combination of considerations connected with anatomy, physiology, and pathology, come in to aid our endeavours to ascertain its nature and origin.

Of these, the first that fall to be noticed are the anatomical facts that the parts of the body where the spots in question usually show themselves, as nearly as possible correspond to the localities, on the chest and abdomen, of the ultimate distribution of the superficial branches of the anterior intercostal spinal nerves; and that the trunks of those nerves, near their origin, hold free intercommunication with the sympathetic ganglia from which the splanchnic nerves are derived.

The second consideration has reference to the aid which, in its bearings on a right understanding of this special point, is contributed towards its elucidation by the true explanation of the painful

sympathetic sensations which are experienced in the shoulder and arm—and even often as far as the finger-tips,—in cases of serious hepatic disease.

Careful study of the symptoms and post-mortem appearances in many such cases that have fallen under his care, in the course of his tropical service, has enabled the writer to determine—as he has fully shown in a previous publication—that the sensations in question are infallible signs of the extension of destructive inflammation from the liver to a neighbouring portion of the diaphragm; that they are the results of injury to branches of the phrenic nerve; and that they are excited by reflex action conveyed along the terminal branches of the brachial plexus to the shoulder, arm, and hand—the inter-communication of nerves which renders this possible existing, in the neck, between the roots of origin of the phrenic and those of the plexus just named.

The fact that in many of the cases which came under the writer's notice, the sensations experienced in the arm and hand were of a very distressing nature, shows the distance to which those influences may extend, and the force with which they may act.

It has already been stated above, that, in all cases showing the presence of a so-called typhoid eruption, well-marked bowel lesions were also

present; and the general evidence recorded as to the time of appearance of the former gives strong reason for concluding that this likewise corresponded with the period of occurrence of the latter conditions.

It seems, therefore, by no means irrational to suppose, that, as in the case of the phrenic nerve and brachial plexus, so in that of the splanchnics of the diseased portions of bowel and the anterior intercostal nerves, influences might be reflected which would act, through the terminal branches of the latter, on the blood-vessels of the skin, owing to partial inhibition of the action of their vaso-motor nerves, so as to induce there, in certain individuals possessed of a peculiarly delicate organization of the skin, minute semi-phlegmonous points of congestion such as this eruption evidently is.

That the anterior intercostal nerves may act as vaso-motor nerves to the blood-vessels of the parts of the skin to which their terminal branches are distributed, there is not wanting, as has already been shown, evidence which makes it almost a certainty that this is really so; and as we already know by conclusive experiments, that, when stimulus is applied to a cerebro-spinal nerve communicating with a sympathetic ganglion, inhibition takes place in the sympathetic nerves connected with

that ganglion, may it not therefore be possible that, when an exciting influence is conveyed from a sympathetic nerve through its ganglion to a nerve belonging to the cerebro-spinal system—the process of communication being exactly reversed, as in the case of splanchnics conveying nerve influences to the anterior intercostals—a similar form of inhibitory action may likewise be produced in the latter?

The explanation just offered of the nature and origin of the spots which appear on the skin of the chest and abdomen in connexion with enteric lesions, whether the true one or not, certainly does less violence, the writer cannot but think, to the teachings of physiology and pathology, as those branches of medical science are understood at the present day, than any other that has yet been brought forward.

In concluding the consideration of the relations which the bowel lesions in non-contagious fevers bear to other symptoms of those complaints, and also to like conditions met with in other forms of disease, the writer would endeavour strongly to impress on the minds of his readers his belief, that, although those states of the intestinal glands now being referred to, are met with more frequently in non-contagious febrile diseases than in any other complaints—or, at all events,

attention has been more closely directed to their occurrence in those disorders—this form of complication is in no way peculiar to that class of diseases.

It may occur in any other form of febrile complaint in which an analogous condition of the circulation in the abdominal organs is set up through the influence of a peculiar state of the cerebro-spinal nervous system. The existence of the condition so set up is manifested, outwardly by the occurrence of increased elimination of urea from the kidneys—those organs, as we have the strongest reasons for inferring, merely participating in the general over-activity of circulation and the results natural to that state, which have been originated within the abdomen in the way above stated.

The one, and only, condition which would appear to be essential to the production of the lesions in question, is, therefore, such a degree of persistence of this state of the circulation (as it affects more especially the intestinal part of the abdominal contents), as will be sufficient to give the time required for the progressive changes to take place in the glands, which eventuate in those peculiar lesions.

Accordingly, the actual occurrence in scarlet fever of intestinal lesions identical with those

hitherto held to be peculiar to enteric fever, has already been recorded by Dr. John Harley in *The Lancet* for December 23rd, 1871.

In the course of his paper that writer makes the following remarks :—

"From this view *one general conclusion* is inevitable—viz., that the pathological changes accompanying an ordinary attack of scarlatina include all those of the first stage of enteric fever, and that the transition from one disease to the other is but a natural pathological sequence, readily determined by any cause which may increase the intestinal irritation." And again—"Nor will I allow this opportunity to slip me, but in the interests of truth will ask my fellow-laborers to go with me one step further, and to discard those transcendental ideas of enteric fever which make it a disease *per se*, and to open their minds to receive what nature will then soon teach them—viz., that enteric fever and all its attendant phenomena may occasionally become a part of almost any other more general inflammatory condition, specific or simple."

The facts which Dr. Harley has recorded give support to the present writer's propositions; but the inferences which the former observer deduces from them, although tending towards the same end, are not identical with the arguments which the

latter would base upon a consideration of all their bearings.

The few remarks that will have to be offered in regard to the information to be derived from the consideration of the conditions met with in the kidneys, liver, and spleen, will be made more conveniently, and in a way that will prevent repetition, further on, when the questions of temperature, and of waste of tissues of the body, fall to be considered.

One other series of important symptoms—namely, those connected with the conditions met with within the head, and with affections of the cerebro-spinal nervous centres of a more serious nature than any that have yet been noticed—still remains to be discussed.

The evidence afforded by the fact of the intellect continuing unclouded, and of all the ordinary faculties of special perception, sensation, and voluntary motion, remaining unimpaired, from the time of the earliest indication of any degree of departure from health, in non-contagious fever, up to the setting in of the stage of re-action, or to the period of death when that happens in the stage of collapse as seen in cholera, demonstrates that the initial functional injury of the cerebro-spinal centres—although of a very serious nature, judging by the results it may lead to,—yet falls

short of that more marked affection of it which must have happened to cause impairment of all those faculties, and induce in them a state of action hostile to those displays of short-intervalled periodicity in nerve impulses which have been shown to be inherent in the nerve-action connected with the healthy functions of organic life, and which are essential to their due maintenance.

The mode in which the power of imparting those intermitting impulses begins to show signs of impairment, would seem to be by each individual act becoming more violent in degree, and prolonged in duration, until, at last, the active portion of the impulse becomes continuous—a condition the attainment of which would appear to imply that serious textural or other injury has happened to the cerebro-spinal centres.

Practically, as will be better understood further on, it is of the utmost importance that the existence (after its onset has become well marked) of a stage of fever which may virtually be held to precede the occurrence of any serious injury of the nature above indicated, should be fully recognized, and its limits of duration as sharply as possible defined. This stage may, for all practical purposes, be said to close, as a general rule, with the completion of the first fit of re-action that occurs after the initial rigor, in all but the very

mildest forms of fever, when the attack is not, at the outset, to prove an abortive one.

Up to that time, the injury done to the great nerve-centres would appear to be functional only (using this mode of expression in a sense implying injury of a strictly temporary and transient nature), and there is reasonable hope that prompt and appropriate treatment may not only prevent its ever becoming of a more serious nature, but may even be the means of causing the progress of events to be arrested, their order of advance inverted, and the conditions of health returned to by the steps of the process, through which those were originally departed from, being retraced.

Once, however, this stage of simple functional injury has been passed, the influence exerted by medicine becomes less and less apparent, until, at last, the ultimate result comes almost entirely to depend on whether the powers of the constitution, aided by judiciously administered support, and by careful nursing, may be able to maintain life sufficiently long to enable what trace there may still remain of certain governing laws bound up with the inherent vital instincts (if such an expression may be used) of the human organism, —the sum of which, as expressed in the equally balanced performance of all the functions of the

body, constitutes health,—to begin to re-assert itself.

Owing to attention having been, for many years past, diverted from those parts of the body whence the first beginnings of the chain of sequences constituting fever proceed, to the more strikingly obvious, but, nevertheless, still purely secondary intestinal lesions, we know little or nothing of the minute changes which take place in the textures of the brain and spinal cord in such cases.

Beyond abundant evidence of the presence of the coarser changes, such as congestion; of the existence, at times, of some degree of inflammatory exudation; and of the nearly constant occurrence of effusion of serum into the sub-arachnoid spaces and ventricles of the brain, we have no records of more delicate observations which, if systematically made, might, perhaps, not only have assisted us to arrive at some definite conclusion as to the conditions which have led up to the more serious lesions of those textures of which we have such strong grounds for inferring the existence at one period of the complaint, but might, also, have been the means of supplying some clue to the nature of those lesions themselves.

The rapidity with which insensibility from extreme congestion of the blood-vessels, and the effusion of even a moderate amount of serum

within the cranium, may supervene; and the manner in which those occurrences are seen to manifest themselves, in cases of sudden onset, in association with intense re-action; and the fact, also, that in cases where this symptom comes on more slowly—becoming gradually developed, and deepening in intensity, with each repetiton of the hot stage of the complaint;—all make it probable that what happens under our eyes, at the surface of the body, in regard to the occurrence of congestion of, and the escape of fluid from the blood-vessels of the skin, is taking place, also, at the same time, and as part of the same process, within the cranium.

In other words, congestion of the cerebral vessels, and a *sweating* on the brain, are happening, within the head, as a consequence of that relaxation of the muscular coats of the smaller arteries which constitutes re-action, and which naturally follows the opposite state of contraction in which the essence of rigor consists;—with this important difference, however, that, whereas at the surface the fluid effects a harmless escape, that effused within the cranium, having no means of exit, accumulates till it causes, by its pressure, loss of consciousness and serious impairment of the functions of the nervous system.

The amount of effusion which takes place

within the cranium, together with the pressure which the intense congestion of blood-vessels leads to is sufficient to cause death, either suddenly, or in the course of a very few hours, during great heat of the weather,—and at other times also, in extreme cases.

In less severe forms of the disease, on the other hand, progress towards the same mode of fatal termination is made more slowly, but with not less certainty, by repetitions of milder attacks of the same process,—the effects produced deepening with their every recurrence.

It may be, that the condition of congestion of vessels and effusion of serum is sufficient to explain the mode of death in all cases ending in coma; and, possibly, the existence of a mild degree of congestion of the tissues of the brain and spinal cord, may be all that is necessary to produce there the initial functional injury upon which the original lighting up of the whole series of phenomena constituting non-contagious fever depends. Be that, however, as it may, one thing is tolerably certain,—that, in no case where death is the result of genuine coma, and where the body has been carefully examined, is evidence ever wanting of the presence of great sanguineous congestion, and, as a rule, of more or less effusion of serum within the cranium.

There can be very little doubt, also, from a study of the symptoms manifested during those periods of the disease, that the actions which take place at the surface of the body and within the head, during both the hot and cold stages of fever, are coincident as to time, and similar in character, in both those situations; and that the effects produced in the latter locality are excited through branches of cerebro-spinal nerves (or of sympathetic nerves containing a large proportion of cerebro-spinal fibres, and exercising functions similar to those with which the nerves supplied to the surface of the body are endowed)—the process being, as regards what happens during the cold stage, in all probability, exactly the reverse of that which is taking place, simultaneously, within the abdomen under the immediate influence of nerves of the sympathetic system.

Leaving altogether out of consideration those cases of pure sunstroke in which insensibility is suddenly induced by exposure to the sun's rays, and which partake more, at the outset, of the symptoms usually seen in pure concussion of the brain, the writer has to express his firm conviction,—founded on his own observation, and on a study of the evidence available on the subject,—that the majority of those appallingly sudden attacks, (now returned under the heads either

of sunstroke, or of heat apoplexy), are, simply, examples of congestive seizures such as have been dsecribed above, occurring at the outset of the hot stage of ague, either at the beginning of an original attack, or in the course of the further progress of the disease.

Those accidents happen, in the majority of instances, during the prevalence of unusually hot weather; they frequently show themselves under circumstances of exhaustion from great bodily fatigue (their most terrible manifestations have occurred during hot-weather marches); and the subjects most liable to their attacks are the unacclimatized, whether young or old, but more especially such of the latter class as may have been of intemperate habits.

The history of all such cases shows, that, previous to the violent seizure, there has always been present more or less of a preliminary stage, corresponding to that witnessed in ague, during which frequency of micturition—one of the signs of approaching ague specially noticed by the older writers—was found to be rarely absent.

The majority of such of those attacks as are recovered from, usually show, eventually, symptoms of whatever form of ague may be prevalent at the time; and it is further worthy of note, that the treatment which has proved the most successful

has been that which included, with other means, the hypodermic injection of quinine.

Men in whom the hot stage of fever has supervened on the cold fit, in the course of their removal to hospital, have been found dead on their arrival there; men, when in bed in hospital under treatment for fever, are frequently attacked, in this sudden way, in the course of a fresh paroxysm of their complaint; and the writer has seen death happen in a similar manner—a mode of fatal termination by no means uncommon in this disease—on the setting in of reaction in cholera. He has even witnessed a like occurrence at the outset of the maturative fever in confluent small-pox, during hot weather.

The evidence given above, the writer believes, when considered in conjunction with the significance of the fact, that post-mortem examinations rarely, if ever, fail to reveal, in fevers ending—whether suddenly or more slowly—by coma, the presence of congestions and effusions within the head, to all appearance identical with those seen in cases of sunstroke or heat apoplexy, can scarcely but be accepted as proofs that his view of the nature of those attacks is not far wide of the mark.

From all that has already been said explanatory of the nature and phenomena of non-contagious

fever, the writer hopes that the important part of his task which still remains to be completed, —namely, that of demonstrating that cholera is simply the highest expression of form which this description of disease attains, will now have become comparatively easy of accomplishment.

Table No. 8, given at page 41, shows, that, like the more aggravated forms of fever, cholera may occur sparsely, and appear in any one of the twelve months of the year. Seasons which, like that from which the materials for the table quoted were taken, exhibit but little epidemic prevalence of this complaint, do not show that its occurrence is usually, in such instances, much more marked at one period of the year than at another. But, when serious epidemic outbreaks take place, it happens with cholera, as with the manifestation of the more severe forms and greatest prevalence of fever, that those occur, as a rule, in different parts of the country, at—taking one year with another—special times, the recurrence of which, although they are not the same for all, can generally be predicted for each station with a tolerable amount of accuracy.

Those periods of most serious prevalence fall, generally, however, within that interval comprised between the approaching close of the rains—when the latter become intermittent, and a close, steamy

atmosphere prevails—and the time when the cold weather may be said to have fairly set in.

It is then that the most marked outbreaks of cholera usually prevail with their greatest severity, in what are commonly spoken of as bad cholera years; which, it must be remarked, however, never fail to be, at the same time, bad fever years also.

In fever, prior to the setting in of the initial rigor, there exists, as has already been explained, a period of departure from health, of from twelve to twenty-four hours' duration. In cholera, the existence of a similar stage (not yet so fully investigated, however, as the corresponding one of ague has been), of like duration, has, likewise, been seen to be present in advance of the outbreak of the characteristic symptoms of that disease.

In severe forms of fever, also, the rigor is often accompanied by pains in the back and limbs, which, in the latter situations, amount almost to cramp —they are, indeed, minor degrees of this,—and, in the more intense forms of attack, the cold stage becomes so marked as to be attended with some degree of blueness of the surface, and much shrinking of the textures. In fact, so far are those special symptoms often seen to be carried, at particular seasons, in the most unhealthy parts of the country, —as, for example, in districts of the Punjab, and more especially in Peshawur—that every degree

possible to their rates of intensity may be traced, grade by grade, from that of the cold stage usual to ordinary ague, upwards, through "Peshawur fever," into the worst forms of cholera—the boundary line which would appear to have been set up by medical officers, between so-called Peshawur fever and cholera, being, in reality, that purely imaginary one which is determined by the presence or absence of suppression of urine. The explanation of that phenomenon offered by Dr. George Johnson, and which attributes its occurrence to the fact of the blood having lost so much of its watery portion, through the intensity of the bowel discharges, as to have rendered the further secretion of urine impossible until some degree of equilibrium between the amount of watery elements in the blood and in the tissues of the body shall have been re-established, seems to be a very rational one, and quite in accordance with the conditions known to exist. This view of the matter would, however, still further tend to show, that the symptom in question is merely an indication of the existing degree of intensity of a special condition, and that it cannot correctly be made use of to mark any such separation into two distinct diseases, as the adoption of such a course would virtually be, without doing violence to the continuity of what is simply a natural sequence of events.

The descending chain—from cholera through Peshawur fever, and back to ague—may, on the other hand, be traced by actual observation of the forms of disease occurring at the time in a particular station, link by link, from the climax of the outbreak, down to, and onwards from, the close of a cholera epidemic.

A similar shading off of the intensity of the prevailing diseases, has also been manifested, after another fashion, as respects a district of country, in regard to the co-existence of all the various degrees of their intensity displayed at different parts of it. Evidence of this having actually happened in the Punjab, is shown by the fact, that, whilst severe cholera was raging at Peshawur, all the various shades of disease referred to above, have been reported as existing at various stations, in zones on a radius extending from the latter place onwards towards Mooltan, and in a degree of intensity which was, in a general way, in inverse proportion to the distance from Peshawur.

In support of the statement that has just been made, the writer will now adduce the following evidence from annual reports by medical officers that have come under his notice. They refer to the year 1869—the last bad cholera year for which annual returns have, up to this time, been furnished;—and they have reference to the stations

of Peshawur, Nowshera, Rawul Pindee, and Mooltan.

As the opinions expressed by some of the medical officers have a still more important bearing on the question of the nature of cholera, the whole of the extracts are given as they stand in the original reports.

1. In remarking on the state of health of the 38th Regiment stationed at Peshawur during 1869, Dr. Whylock, surgeon of that corps, makes the following observations :—

"With regard to health, nothing good or favourable can be said of Peshawur, it being certainly one of the most, if not the most, malarious and unhealthy occupied stations in all India. Miasmatic disease is particularly prevalent at all seasons, but more especially about September and October. Ague then prevails to an enormous extent; the early and later cases assuming all the appearance and symptoms of cholera, and, to the uninitiated, being very alarming. There is every range of fever, from ordinary intermittent to remittent and typhoid. . . . . .

"*Ague.*—Of this disease there were 1,365 cases, showing every imaginable variation. The most remarkable of these cases so extremely nearly resembled cholera, that had we not been prepared by previous knowledge of the peculiarities which

presented, particularly as cholera had only just ceased, we should have taken it for that disease. There was collapse; cramp in extremities, and in muscles of abdomen and chest; cold blue tongue; vomiting of a peculiarly clear fluid, just, in many cases, tinged with blood, and without smell; loose, copious white stools; almost pulselessness; coldness of skin; thirst; sunken features. In no case, however, was there absence of urine. The first symptom was, usually some hours before, peculiar pains in the toes. When reaction was thoroughly established, there was no sign of a return of the disease, but great depression and debility resulted: and now came on regular and severe attacks of well-marked ague."

2. With regard to the sickness at Nowshera, Surgeon A. H. Fraser remarks, in the annual report of the 88th Regiment which was stationed there in 1869,—"General diseases—sub-division A. —chiefly fevers, have been the cause of more than two-thirds of the total admissions, taking them in the order of arrangement. The deaths under this sub-division have not been numerous compared with the admissions. Three cases of enteric fever were admitted, one of which proved fatal. It was supposed that some local cause existed, which would account for the appearance of these cases, but although the most careful investigation was

made, nothing could be discovered to which their appearance could be attributed. Simple continued fever caused 164 admissions and 2 deaths. Paroxysmal fevers show 919 admissions and 3 deaths. This form of disease, which has been unusually rife throughout the Punjab, prevailed chiefly in October, November, and December, both in Nowshera and Attock. Although mild in character on first appearing, as the season advanced it assumed a grave character, and the paroxysms were characterized, in many instances, by extreme depression and tendency to collapse, diarrhœa, and troublesome vomiting, in some instances of sanguineous matter. The patient would become cold and pulseless after a few watery evacuations. After reaction set in, sleeplessness from extreme nervous exhaustion was frequently present. The liver and digestive system became involved much oftener than the spleen. . . . . .

"Cholera has touched most stations in the Punjab with great severity during the past year, and, in its wake, fever has been much above the average. When the one appears the other is sure to follow, as if a certain connexion existed between them, both depending on climatic peculiarities. Cholera caused 2 admissions and 2 deaths. Although only 25 miles from Peshawur,

Nowshera, with the above exceptions, escaped the scourge."

3. At Rawul Pindee, Surgeon-Major Hassard, 1st Battalion, 19th Regiment, speaking of the health of the corps for 1869, makes the following remarks :—

"Until the middle of September the regiment enjoyed most excellent health, when fever of an ardent type attacked the whole of cantonments in an epidemic form, and in many instances accompanied with alarming symptoms, such as obstinate vomiting, watery diarrhœa, and collapse, assimilating in many instances, the first stage of cholera. When these symptoms were prevalent at Rawul Pindee, cholera was decimating the troops at Peshawur, and, as the wells at Rawul Pindee, owing to the long drought, were very low, I considered it advisable, as a precautionary measure, to recommend, that all water forcooking and drinking purposes should be boiled, and I may here remark that the cases of fever admitted to hospital after the issue of boiled water were certainly milder in type, and the irritation of stomach and bowels considerably less. Towards the end of September after a heavy fall of rain, the type of this epidemic fever suddenly changed from ardent continued to simple ague which, although not so formidable in symptoms, by its repeated occurrences, rendered

many of the men weak and anæmic for the rest of the year."

At Rawul Pindee, also, Dr. W. H. Jameson, Assistant Surgeon, Royal Artillery, makes the following remarks in the annual report of E. Battery, 19th Brigade :—"Fevers of the periodic type have furnished 44·79 per cent. of the whole number of admissions, viz.,—258—247 of these being for ague, and 11 for fever of a remittent character. The former was, also, directly the source of invaliding in one case.

"The E. 19th Brigade, Royal Artillery, in common with almost all troops in Rawul Pindee, suffered during the months of August, September, October, and early part of November to an extent hitherto unprecedented in this station. The variety of intermittent most usual was quotidian, verging into tertian on recovery. The symptoms were at the onset commonly the characteristic rigors with great gastric irritation, diarrhœa accompanied by watery evacuations, and considerable pain, and frequently cramps of the abdominal and extensor crural muscles, and, in some instances, rather alarming collapse and cold clammy surface, presenting no remote analogy to cholera.

"In fact, reviewing the sad visitation in the neighbouring garrisons, and observing there the fluctuations of the two diseases, their close affinity

has been evidently indicated. The bare fact of statistics shows the vicarious relation of malarious fever and cholera—the rise and fall in the number admitted for each in places where cholera raged being always diametrically opposed, appearing as if the graver disease temporarily supplanted the allied malarial fever. The deduction that the causes operating in both cases were the same, does not seem far fetched—the degree of intensity merely varying according as the locality was more or less favourable to the production or perfecting of the poison whether in the form of emanations from the soil, evolution of germ-growths from the sub-soil, or some atmospheric source. The emesis was at the onset of the attack almost uncontrollable—sinapisms to the epigastrium being found to be the most useful palliative—quinine in full doses being the only reliable curative agent."

4. In regard to Mooltan, Assistant Surgeon Ussher remarks, in the annual report of the 109th Regiment quartered there in 1869, as follows:—

"Intermittent fever caused the largest number of admissions. During the last three months of the year under report, malarious fevers prevailed epidemically, and officers and men alike suffered severely. The type of the epidemic was anæmic —there was great nervous prostration accompanied with copious bilious vomiting and purging.

Stimulants in the form of wine, brandy, carbonate of ammonia, and chloroform were largely given with the best possible results. The cause of the outbreak of the epidemic is uncertain, but may be referred to the unusually large rainfall at the station during the year."

It will not, perhaps, be out of place to record here the very latest independent contribution of evidence in support of the writer's views, which has come under his notice whilst this essay is passing through the press. It is contained in the following extract from the remarks on the return of sick of the 105th Regiment, stationed at Meerut, for the week ending the 6th September, 1872, and is from the pen of Surgeon Haward of that corps :—

"For the last week cholera has prevailed in an epidemic form. Eighteen cases have been admitted, and of these 13 proved fatal. The regiment is now broken up into detachments, and encamped at different places. Some of these camps have suffered less than others—the disease being apparently aggravated by any and all circumstances that at other times produce fever; and whatever malaria may be, there can be no doubt that, in the present instance, it is aggravating, if not originating cholera. Cases are admitted of men who have suffered from ague, and in the cold stage purging

and vomiting commence, the disease suddenly assumes the form of cholera, the stools are characteristic of the disease, collapse suddenly ensues, the skin becomes sodden, clammy and cold, the voice fails, and the pulse becomes imperceptible. The only two symptoms of malignant cholera that in these cases have been indistinctly marked or altogether absent, are cramps and restlessness. One case occurred of a man, who rallying from cholera immediately assumed the symptoms of remittent fever, heat of skin, pulse 110°, delirium, passing stools like yellow ochre. In all cases, the poison appears to be received during the night, the first symptoms appearing soon after daybreak."

It has now, the writer hopes, been pretty clearly shown by the facts he has adduced above, and by the independent statements of other medical officers which he has brought forward in support of his own views, that the various degrees of rigor—ranging from its slightest manifestation seen in the mildest form of ague, to the deepest collapse of cholera—are but shades in intensity of one and the same set of symptoms.

There still, however, remains to be noticed the characteristic vomiting and purging of the cold stage of cholera; and it now falls to be considered whether those phenomena, also, may be explained on the same principle of simple intensity of

degree which has, the writer trusts, been not unsuccessful in meeting the difficulties attending the investigation of the intimate nature of the more ordinary symptoms common to the milder rigor of ague, and to the deeper collapse of cholera.

Here, also, the writer feels free to maintain that the results of more recent physiological experiments do not fail to aid us; and he would affirm, that by their help we are enabled satisfactorily to dispose of what would otherwise have proved an insuperable difficulty, and one which would have left such a hiatus in his evidence, at a point of so great importance, as would inevitably have been the means of invalidating the whole of his conclusions. As the matter can now, however, he hopes, be put, the writer cannot but think that the explanation which is about to be offered in regard to the nature and origin of this hitherto inscrutable phenomenon, will go far to make it one of the strongest links in his chain of evidence.

It is well known, that, as a rule, the bowels, in the milder forms of ague, show, at the outset of the disease, an inclination rather to constipation than otherwise. Later on, however, and more especially if the complaint threatens to assume any of its graver aspects, a tendency to looseness of the bowels begins to manifest itself. Moreover, when from peculiarity of season, or from other

cause favourable to an unusual prevalence of fever, and to the assumption of its higher shades of intensity, the disease, as has already been shown, displays its more severe types, diarrhœa—varying as to degree from simple bilious diarrhœa, to watery discharges bordering on, and gradually passing into, the rice-water stools of cholera—is very frequently met with.

This matter of purging as an accompaniment of fever, is, in like manner, in all probability, merely a question of the degree of intensity of exciting nerve-influence—the amount of inhibition of the abdominal sympathetic which may have been called into play, determining whether there shall occur only some degree of moderate congestion (in other words, increased activity of circulation) of the intestinal mucous membrane, such as there is every reason for believing exists at the outset of the milder forms of ague; or, whether the exciting influence shall be of so marked a nature as to lead to those more intense forms of that condition which are, in their highest degrees of expression, accompanied by an outpouring of the watery part of the blood from the relaxed and dilated intestinal blood-vessels, of which there is found such unmistakeable evidence in cases dead of cholera. That the more intense bowel discharges which occur in the cold stage of cholera, should

indicate the presence of the highest possible degree of inhibition of the vaso-motor nerves, would naturally be inferred from a study of all the conditions attending fever and cholera; and, accordingly, the following indirect proofs that this is really what happens, may, reasonably, the writer believes, be held to be as conclusive evidence in regard to the point in question, as it is well possible to desire;—probably, at all events, as complete as we can ever hope to be able to procure on such a subject.

Inhibition of nerve-action is, as has already been explained, the existence of a condition in a nerve resembling the paralysis it would suffer were it divided across—with the difference, however, that this equivalent of paralysis is merely of a temporary nature; that it may vary in degree according to the intensity of the controlling nerve-influence causing it; and that it will gradually disappear, altogether, as the latter ceases to act.

As Moreau's experiment, already referred to, in which a condition of bowel discharge in every way identical with that witnessed in regular cholera, followed division of the roots of the sympathetic nerves supplying the intestines in dogs, reasonably demonstrates, that, as actual division, and, consequently, complete paralysis,

are the equivalents of the highest degree of inhibition which can possibly be produced; so, the occurrence of precisely similar bowel discharges in cholera, where there has been no permanent destruction of the nerves, makes it almost certain that the highest degree of inhibition which is ever attained to in disease, must have been set up in that complaint.

Taken in connexion with all that has already been advanced in relation to the subject of the identity of those diseases, this last consideration can scarcely fail to be accepted as a very strong proof that cholera is but the highest expression of form of non-contagious fever; and when the experiment of Moreau shall have been successfully repeated by other observers, the accuracy of this view of the whole matter will, the writer would fain believe, have been placed beyond the reach of reasonable cavil.

Further testimony favourable to the accuracy of the views expressed above regarding the nature of the phenomena of various shades of the cold and reactive stages of fever and cholera, may be drawn from the conditions which attend the gradual subsidence of collapse, and the establishment of reaction in the latter disease.

The first sign of the approach of the stage last named is a gradual abatement of both vomiting

and purging, thus showing that the state of inhibition within the abdomen must have begun to relax. With this comes evidence of a gradual restoration of the circulation at the surface and extremities as is shown by the pulse becoming more distinct; by a rise of the temperature of the surface (the mode of this increase being identical in cholera and ague, as proved by thermometrical observation); and, by the disappearance of the blue and shrunken condition of the skin;—all evidence that, simultaneously with the relaxation of inhibition within the body, the influence which led to the coincident constriction of the blood-vessels of the surface is also ceasing to act.

The gradual steadying in its movements, and increase in the force of its action, which, at the same time, take place in regard to the heart, show also that this one common influence must have been exerting its force on that organ likewise; and that its withdrawal is being kept pace with by the subsidence of the other changes of action which had, up to then, been manifesting themselves in regard to the circulation generally, —although after different fashions in respect of different parts of the body.

The occurrence of the well-known signs of disease above alluded to, and the possible recognition through their means of the existence

of the various internal conditions of which they indicate the presence, are all evidence that can scarcely fail to be accepted as so many fairly conclusive proofs of the general accuracy of this mode of explaining the various phenomena which have now been passed under review.

Army medical officers, and medical men generally, have apparently often been, to a considerable extent, misled in their conclusions as to the nature of cholera, by an error in reasoning, which, although, at first sight, it seems a natural one to fall into, is yet an unfortunate one to commit, inasmuch as the train of conclusions to which it has usually conducted them, has carried them wide of the path by which alone it would appear that any rational explanation of the nature of cholera can, by any reasonable possibility, be reached.

The error in question is that which leads them to connect with the suppression of urine which generally takes place in the advanced stage of collapse,—and which continues for some time after the setting in of reaction in cholera,—the alarming head symptoms that so often suddenly supervene on the establishment of reaction, and to set them down as signs of uræmic poisoning consequent on the non-elimination of uræa and uric acid by the kidneys.

A far more rational explanation of the cause of

cessation of secretion by those organs, has already been given in attributing it simply to that loss of its watery portion which the blood sustains through the sudden and excessive bowel discharge. Besides, our knowledge of the conditions under which uræmic poisoning occurs in connexion with other diseases, makes it most unlikely that this complication ever arises in a manner so sudden as would be implied by an occurrence such as that above alluded to.

The only explanation of the phenomena of this stage of cholera which comes at all true to nature, is, therefore, that which accounts for the head symptoms of reaction by looking upon the latter condition as simply the hot stage of fever, occurring in that high degree of intensity it was to be expected it would necessarily show, when following on a cold stage the deepest that can ever occur short of actual death.

Not less unfortunate, also, the writer regrets he cannot but feel bound to believe, have been the views expressed as to the condition of the lungs met with in fatal cholera. The statement which has been put forward, to the effect that the condition of those organs is, under such circumstances, always one of anæmia when death has taken place in the stage of collapse, is simply opposed to the facts as they present themselves in connexion with

the malignant cholera of India—the condition of the lungs found after death at that stage of the disease, in this country, being always one of much engorgement.

Moreover, the writer fails to discover in this increased vascularity of the pulmonary tissues, anything beyond the result which, whilst the influences leading to cholera continue in activity, must inevitably follow that inhibition of their vaso-motor nerves under which those organs, in common with every other texture within the chest and abdomen supplied by the great sympathetic system of nerves, cannot but fall.

Later in the disease, when death has occurred from the head complications which are set up during reaction (now known as the stage of secondary or consecutive fever), the engorged state of the lungs found then, is—like that which is present after death in cases of sunstroke or heat-apoplexy—simply the condition usually met with there in all cases of death beginning at the head, and which must necessarily attend all fatal terminations which happen in that way.

Had the pathology of the different modes of dying (a right understanding of which is so essential to the successful investigation of disease) been allowed more weight by previous observers in this line, the mistake of applying the term "heat

asphyxia" to so-called sunstroke and heat-apoplexy, would probably never have been committed; and perhaps, also, the pathology of cholera, as that has reference to the condition of the lungs, might have been more clearly made out.

As regards the post-mortem appearances discovered in the intestinal canal and abdominal organs generally, in cholera, the evidence is more limited than might have been desired. This would appear to be mainly consequent on the views at present entertained on the subject of the nature of cholera which have virtually made that disease to be looked upon, and authoritatively proceeded against—so far at least as India is concerned—as, in every respect, a contagious disorder, the spread of which is to be combated by every form of disinfection available, and its subjects handled with caution.

The facts, however, which have come specially under the writer's cognizance, in respect of the post-mortem appearances, will, he would venture to hope, be found quite sufficient to show, when due allowance is made for difference in intensity of degree of the causes which produced them, that they are very closely in accord with what was to have been anticipated on the view that cholera is simply the highest form of expression of non-contagious fever.

It is now over twenty years since the writer took part in the first post-mortem examination he ever witnessed of a case of cholera. The subject of it was under twenty years of age; and the disease, having shown much malignity, had been proportionately rapid in its course.

The remarkable feature in that instance, was the intense degree of congestion found to exist in the greater part of the intestinal mucous membrane. This condition caused the tissues, when exposed to the air, to assume a colour of so deep a pink as to have conveyed to the writer's mind the idea that he then beheld the most intense degree of congestion, short of that consequent on regular inflammatory action, that he had ever before seen; and, at the time, he formed the belief which has never since left him, that in the occurrence of the condition he had just witnessed, howsoever that was to be accounted for, would be found to lie the explanation of the bowel discharges in cholera.

Subsequent experience has only tended to strengthen the impression he then received. All the records of post-mortem examinations that have passed through his hands, have invariably noticed the presence of great congestion—sometimes in patches, sometimes continuous—of the mucous membrane of the stomach and intestinal canal; some have described portions of the mucous coat as

showing a "sodden" appearance, whilst others, in the cases of longer duration, have noted the existence of enlargement and prominence of the intestinal glands.

Congestion of the kidneys and liver are also noticed; and, in the majority of instances, the gall-bladder is spoken of as having been either distended, or moderately filled, with bile.

One remarkable fact observed in connexion with the pathological appearances met with in cholera, is the reduction of size which the spleen would seem, as a rule, to have undergone in all such cases, and which differs so widely from what is generally found to be the condition of this organ in most diseases that end fatally in the tropics.

In seven cases of cholera in which the weights are carefully given, the average of the whole was only 5·71oz.—the average weight of the healthy spleen, between the ages of 20 and 30, having been authoritatively set down as 7·19oz., for males.

The few facts given in connexion with this subject, on Table No. 5, page 37, would tend rather to connect this shrunken condition observed with the degree of vomiting and bowel discharge which was present during life. The intimate connexion which exists between the blood-vessels of the spleen and stomach, and the fact that

there is some slight evidence to show that the spleen undergoes a considerable reduction in size in the cases of fever attended with marked diarrhœa, as compared with those in which the bowels may have been but little affected, would go far to prove that some such connexion does exist between the state of the spleen and the amount of bowel-discharge.

Be that as it may, the change observed is so marked, and it contrasts so strikingly with the condition in which the spleen is usually met with, that it is impossible to avoid the belief that the right understanding of its cause must be of importance to know.

The bare mention of the facts given above may, perhaps, prove suggestive to those conducting physiological inquiries as to the nature of the hitherto mysterious functions which this organ performs, and of which we as yet really know so little.

Mention has already been made of the condition in which the lungs are found, and nothing more need be said on that subject.

But perhaps the best evidence, so far as it goes, that can be adduced in support of the writer's views, is that having reference to the minute changes met with in the intestines in cholera, which is to be found in the seventh annual report

of the Sanitary Commissioner with the Government of India for 1870, in a special appendix furnished by Dr. Douglas Cunningham, of the Indian Medical Service.

The essay just referred to, forms Dr. Cunningham's contribution to the cholera investigations which his coadjutor, Dr. Timothy Lewis, of the British Medical Service, and he have been specially engaged upon since 1869.

The inquiry, so far as it has yet advanced, has had reference chiefly to the microscopical appearances met with in the discharges in the course of the disease, or in connexion with other conditions which could, in any way, be expected to throw light on the subject under investigation. The facts that up to this time have been collected, have, therefore, perhaps, on the whole, produced only negative evidence in regard to the object for which the investigation is being conducted; but, when considered in connexion with the views which the present writer has advanced as to the nature of fever and cholera, he cannot but think that they afford very valuable indirect corroborative evidence of the truth of the propositions he has been endeavouring to maintain.

The following quotation from paragraph 149 is, he believes, of much importance as demonstrating that the portion of the intestines which shows the

highest degree of diseased action in fatal cases of cholera is identical with that in which the lesions of fever manifest their greatest intensity.

The nature of the morbid changes, too, which are found there, is, moreover, very much in accordance with what might have been anticipated. For, whilst post-mortem examinations would tend to show that loss of the epithelial lining of the intestines is not essential to the occurrence of the watery discharges of cholera, yet—as it seems reasonable to conclude would be the case—this destructive process does actually take place at those points where, as has already been explained, the natural vascularity, and, consequently, the induced congestion, manifest their presence in the highest degree.

The remarks alluded to are as follow :—

"That the destruction of epithelium is, in some cases at least, very limited in extent, I cannot but believe, seeing that in several of the *post-mortems* in which I have had an opportunity of carefully examining the intestines, the evidences of destruction were confined to a small portion—about two feet in extent of the ileum, immediately above the cœcum; for while in this locality the mucous membrane appeared thinned, congested, deficient in follicles, and denuded of epithelium, yet throughout the rest of the

intestine there was no evidence of denudation, and the epithelial lining only differed in appearance from its normal condition in presenting a somewhat macerated aspect."

As regards the existence of any special fungus growth, or, indeed, of any special microscopic appearance peculiar to cholera discharges alone, the evidence adduced by Dr. Lewis in the report for 1869, and by Dr. Cunningham in that which has just been quoted from, is of a distinctly negative character.

With reference specially to Professor Hallier's cholera-fungus theory, Dr. Cunningham remarks (and his observations tally exactly with those previously made by Dr. Lewis), in paragraph 183, as follows:—

"The results of extended series of observations on the cultivation of choleraic materials in various media and under various circumstances of exposure to, or exclusion from, external influences, have likewise afforded not the faintest support to the idea that there is any special form of fungus associated with such materials.

"It is not merely the case that there has been a failure in the appearance of Professor Hallier's cholera series of forms, but a failure in the appearance of any special forms whatsoever."

In respect, moreover, of the conclusion he has

further arrived at, that no special microscopical appearances occur in cholera evacuations which are not found also in discharges resulting from other conditions of the bowels, the following remarks by Dr. Cunningham, contained in paragraphs 206, 216, and 227, will now be quoted :—

"206. The physical characters were naturally very different according to the nature of the case from which the material was derived, ranging from those of normal healthy dejecta to those of a watery fluid with hardly a trace of sediment. The most important point to be noted, as we shall subsequently find in the details of cases of diarrhœa induced by large doses of sulphate of magnesia, is that simple artificially induced diarrhœa may cause the evacuations to assume general characters, precisely similar to those of cholera without inducing a single characteristic choleraic symptom. Such dejecta frequently occur with a colourless or pale-grey fluid, and a sediment composed of white or pale yellowish, gelatinous flocculi, rapidly and completely subsident, and are absolutely indistinguishable in outward appearance, at least from those which are characteristic of cholera. To such a degree is this the case, that on several occasions I have known medical men with a large experience of cholera cases unhesitatingly pronounce such material

choleraic, and only with difficulty convinced that this was really not its nature.

"216. Before proceeding to the consideration of the remaining points in connection with non-choleraic dejecta, it may be remarked that certain of these cases of diarrhœa were characterized by a very profuse and rapid discharge of watery fluid from the intestinal canal, and were yet entirely free of the slightest symptoms of cholera. So profuse was this discharge in Nos. 13 and 14 of the table as to cause syncope in the former, and a tendency towards it in the latter case. In the latter, as previously mentioned, the patient's weight was diminished by lbs. 2 oz. 12 in the course of seven hours, due to the discharge of a watery fluid and of flocculi similar to those of cholera; and yet, with the exception of a tendency to syncope, there was no symptom of cholera present. This loss of weight cannot be ascribed to mere emptying of the intestinal canal, as it occurred after the passage of lbs. 6 of fluid dejecta.

"227. The general conclusions to be derived from the comparison of the characters of choleraic and non-choleraic dejecta appear to be that as yet there is no evidence of the existence of any specific cells, or other bodies peculiar to the former and never to be found in the latter, but

that there is distinct evidence that many of the peculiarities of choleraic dejecta are due to a great increase in the development of certain classes of cells, common to them and to non-choleraic materials, and both intrinsic and extrinsic to the organism, and that, as regards the extrinsic cells at all events, this increased development may be ascribed in great measure to the presence of a favourable medium in the fluid which is effused from the blood."

The whole of the foregoing observations made independently, and evidently founded on very carefully conducted investigations, possess, both actually and inferentially, the writer cannot but think, very important bearings on the subject now under consideration, and afford very material support to the views he has been endeavouring to advance in the course of the present essay, inasmuch as they show very clearly :—

1st.—That in respect to the portions of the intestines chiefly affected in cholera, those are identical with the parts which, it has been distinctly demonstrated, suffer most in fever.

2nd.—That as regards the nature of the lesions found in the two diseases, the apparent difference which exists between them is only one of degree, dependent on the period of duration of the disease, and the intensity of the attack.

3rd.—That, by means of irritation directly applied to the intestinal mucous membrane through the administration of active purgative medicines, bowel discharges may be produced which can in no respect be distinguished from those due to cholera; and that, too, without the occurrence of any of the characteristic symptoms of that disease, beyond those, such as syncope and faintness, which cannot fail to be common to cholera and every other condition in which sudden and great loss of the watery portion of the blood may happen.

In what, then, lies the difference between this watery purging that can be artificially produced, and which, both in its degree, and in its physical characters (microscopical and other), is identical with that of cholera, and that which actually occurs in the latter disease?

We have just seen that by a reflex nerve-action caused by the direct irritation of the mucous membrane of the intestinal canal, a condition of the circulation of its blood-vessels is induced which leads to a watery discharge identical with that of cholera,—the influence exciting this remaining of so purely local a nature as to produce only those discharges and the faintness consequent on them.

Professor Moreau's experiment has shown that in the lower animals a similar condition can be

induced by division of the roots of the sympathetic nerves supplying the intestines. This, again, originating a condition of those nerves which manifests itself through a part of the nervous system which, not only naturally, but as a result of the kind of injury done it in this instance, possesses a purely local influence confined to the parts it supplies with vaso-motor nerves, will, therefore, also induce symptoms similar to those just referred to as having been caused by reflex action locally excited.

In the various degrees of cholera, on the other hand, as the nature of that disease is attempted to be explained on the views adduced by the present writer, the bowel-symptoms, although identical with those which can be—so to speak—artificially produced either by stimulation applied to the terminal nerves of the intestinal mucous membrane, or by division of the roots of the sympathetic supplying the intestinal structures, are but one series of manifestations of the existence of a more general influence which has its origin in the great cerebro-spinal nervous centres, and which affects, not only the nerves supplying the bowels, but re-acts, also, simultaneously—in the way peculiar to each of the structures and parts of the body as that is determined by the nature of the functions of their regulating nerves—on every

organ within the abdomen; upon the heart and lungs; and on the circulation, as well at the surface of the body and extremities, as within the head.

His readers are now, however, he would hope, in a position to readily understand all that the writer wishes to be inferred from what he has just stated, without its being necessary that he should again go over the ground he has already, in the preceding parts of his essay, taken so much pains to endeavour to clear.

Both Dr. Cunningham's observations to which attention has been drawn above, and the evidence in regard to the condition of the intestines in cholera, which has come either under the present writer's own personal knowledge, or has, as the work of others, passed through his hands, have had reference either to the state of the epithelial coat alone, as in Dr. Cunningham's researches; or to that of both the epithelial and vascular coats, as happened in the writer's experience. The simple evidence which has thus been obtained, is, however, quite sufficient to account for, and is perfectly consistent with, all the bowel phenomena of the disease.

That the capillaries of the villi, in parts where congestion has been the most marked, may suffer injury from the intense and prolonged over-distention caused by the excessive hyper-activity

of their functions, which, there is every reason to believe, their delicate walls undergo during the duration of the whole period of collapse, might naturally be anticipated. The only positive evidence, however, that this does actually happen, which the writer has met with, is that adduced by Dr. Lionel Beale in regard to the state of the villi of the small intestines met with by him in fatal cases of cholera, and in which he has delineated a shrunken and wasted condition of portions of the capillaries. He attributes the origin of this to the presence of a poison which he considers must have been in activity for some time previous to the manifestation of the disease.

Believing, as he fully does, that the occurrence of the bowel symptoms in cholera is in no way owing to the presence—in the sense understood by Dr. Beale—of a poison, but is altogether secondary to a peculiar condition induced in the nervous centres, the writer regrets he cannot endorse any view which supposes the existence of a poison acting directly on the intestinal mucous membrane and blood-vessels, either from the intestinal canal itself, or through the medium of the blood.

Distention of the delicate capillaries of the villi alone, might, however, from its excessive degree; from its prolonged duration; and from

all that those conditions lead to, be sufficient, the writer believes, to account for the injuries figured by Dr. Beale. Neither does he think that further time would be required for perfecting the changes described, should Dr. Beale's observation be confirmed by other observers,—doubts as to their accuracy having, in the meantime, it would appear, been raised by another microscopist—than is supplied by the usually recognized period of duration of cholera, and of the time that must have elapsed between the dates of death and those on which the examination of the diseased textures was made.

The writer will now endeavour to bring this chapter to a close by recording the few observations he has to offer on the rise of temperature witnessed in disease; and in regard to that and other points connected with the functions of nutrition and secretion both in health and disease,—a due consideration of those being of the first importance as aids in advancing the elucidation of the subjects to which this essay has been devoted.

The provision that has been made for the growth, maintenance, and renewal of the various tissues of the body, may be said to consist, essentially, in the laying on to every part of the body, through means of the arterial system, of a supply of fluid pabulum—the blood—containing

every material necessary for the supply of the requirements of those processes. The nutrient substances in question, as they exist in the blood, are either in a state of solution, or are present in such conditions of fluidity, and of arrangement of constituents, as admit of their readily undergoing the transformations necessary to adapt them to the special wants of individual textures.

The primary function of the heart and arterial system appears to be of a purely mechanical nature, which is fulfilled when, by their combined action, the blood is steadily passed on to the immediate vicinity of the minute components of every texture in the body, and the blood-ducts are kept filled up to a certain degree of tension.

The blood itself seems to be little more than a complex fluid, obeying ordinary physical laws, and contained in what is virtually a tubular, colloidal membrane, through which a vital osmosis is ever taking place, in one direction or the other, between the contents of the blood tubes, and the constituents of the contiguous textures—the latter being made up of some one or more of the various elements contained in the blood, only somewhat transformed.

The more purely mechanical processes which the heart, the arteries, and the blood, may be said to directly fulfil, are, however, supplemented

by the action of the vaso-motor nerves, which induces in the coats of the blood-vessels of the different textures of the body, conditions that lead either to a diminution and retardation, or to an increase and acceleration of the flow of blood through particular parts, according to the nature of the influences—either direct or reflected—they may receive from the cerebro-spinal centres, or great sympathetic nerves, either under the ordinary conditions of health, or in those of a more special nature connected with disease.

If we suppose the body, so far as voluntary motion is concerned, to be at perfect rest, and the performance of all its ordinary animal functions to be progressing with uniformity, we can then understand how simply physical in their nature the changes necessary to maintain the nutrition and renovation of the tissues through the action of osmosis are—the constituents of the blood being all the while maintained in a state of equilibrium by the addition of new watery and other materials, the result of assimilation and absorption; by the removal from it of substances required for nutrition of texture, and for the maintenance of secretion accessory to vital processes; and by the withdrawal, through the more specially excretory channels, of those that have become effete or superfluous.

Bearing all this in mind, and remembering that

no change of this nature takes place within the body without an evolution of heat attending the process, we will be able to understand how the transformation of the colloidal materials of the blood into tissues and important secretions; or, of its crystalloidal ones into the various excretions, maintains the temperature of the body at the nearly uniform rate which has been set down as that of ordinary health.

All observations hitherto made on this subject, would, moreover, seem to show, that, although the blood may possess a larger proportion of the elements which go to form a special secretion at one time than at another, yet in no instance does it really contain any secretion actually formed. Neither has the blood any power to force upon a particular organ, by a purely local influence exerted at the seat of manufacture of a special secretion, the action which will cause the removal from it of any particular set of its components.

The latest physiological experiments, and pathological observations, go to show, that neither the presence in it of a poison, nor of any unusual material that the blood may contain, acts directly on any organ. The existence of such conditions can only become the means of influencing the functions of the latter in an indirect manner through the effects which, in some, as yet, unexplained way,

the circulation of blood so constituted produces on the nutrition or functional action of the great nerve-centres.

The action thus induced is, in regard to the blood-vessels of all the organs contained within the chest and abdomen, and in respect of such others as are accessory to the functions performed by the intestinal canal, of a purely inhibitory nature, causing dilatation of minute arteries; an acceleration of blood-flow through them; and an increased activity of the ordinary nutrition and special functions of the parts they may supply, which will be proportioned to the degree of the process of inhibition that may have been brought about.

There is also no evidence which can be made to show that any real grounds exist for the belief hitherto current, that such changes may be set up in the tissues of the body, through the direct action of nerve-influence on their constituents, as will load the blood with their effete products, and lead the latter, on its part, to force those on the organs specially adapted for their removal. All the facts known in connexion with this subject go to prove, on the contrary, that it is the condition induced in the nutrition and functional activity of the nervous system through the state of the blood circulating there, that, by exercising

inhibitory powers over the sympathetic vaso-motor nerves, brings about an unusual flow of blood in a particular organ; sets up undue activity there; and pushes the removal of the special material which it is adapted to withdraw to such a degree as to disturb the equilibrium of the blood constituents, reverse the ordinary action of osmosis, and so, eventually, to draw, through means of that process, upon all the tissues of the body which contain the special element in question in the least stable condition, in order to meet the unusual expenditure which the hyper-activity of an organ thus forced upon it by the state of the vaso-motor nerves of its capillaries, has set up.

It is only in this manner, the writer believes, that the increased discharge of urea and uric acid, and the wasting of the tissues which occur in ague, are to be explained in a way which shall not only be true to nature, but, at the same time, in accordance with the results of the latest physiological experiments bearing on the subject.

It is, of course, in respect to what happens in the case of the kidneys, that reference is chiefly made in speaking of the increased elimination of nitrogenous materials which takes place in ague. Post-mortem and other evidence is, however, so clear on the point, as to leave no room for doubt that a corresponding increase of activity in their

functions, is, at the same time, happening in this disease to all the secreting organs within the body.

It is, moreover, the increased tissue changes, and the augmented transformation taking place in the act of exosmose—from the blood-vessels to the organs—in the case of this extra functional activity of the latter, as well as the corresponding acts of a like nature which occur in the endosmosis—from the tissues back to the blood—which the first named demand on the constituents of the latter leads to, that cause the increase of temperature that occurs within the abdomen during, and probably, also, previous to the existence of the cold stage of ague.

The apparent contradiction found to exist between the occurrence of increased ureal elimination coincident with a low temperature at the surface of the body, has already been explained by the different results which follow, simultaneously, at the surface of the body, and within the abdomen (contraction of blood-vessels in the former case, relaxation in the latter), on the action of the primary exciting nerve-influence proceeding from the cerebro-spinal centres.

Like those of all other parts within the body, the intestinal capillaries suffer an increased activity of their circulation in ague, with results which have already been detailed. In the highest degree

of this state of the blood-vessels which is witnessed in the stage of collapse in cholera, the intestinal capillaries which had, at first, owing to turgescence from over-activity of their circulation, probably suffered arrest of their habitual absorbing powers, come, at last, through the degree of intensity to which the latter condition attains, to have their action reversed to such an extent as to make them channels through which the watery, and eventually other more important parts, also, of the blood, are poured out with a rapidity unequalled in any other state of the body; and which only ceases when inhibition of nerve-influence within the body comes to a close—an occurrence indicated, at the surface of the body, by the setting in of reaction—not, however, before the loss of its watery portion sustained by the blood has rendered the secretion of urine, in the more intense cases, impossible, and has reduced the action of the kidneys to the production from the uriniferous tube-lining, of that milky deposit of epithelial debris with which the pelvis of each of those organs, under such circumstances, usually becomes filled—evidently in consequence of a lack of the fluid part of the secretion in which the epithelial products in question are, usually, either dissolved, or washed away.

It has been for some years past maintained, on

the strength of appearances revealed by the microscope, that so much are the capillaries of the kidneys liable to be influenced by poisonous or effete materials that may exist in the blood flowing through them, that their muscular coats, through direct excitement to contraction thereby induced, have actually become hypertrophied in the true meaning of that term as applied to muscular tissue.

The writer cannot but think that this is a view of the nature and origin of those changes which it will be found difficult to reconcile either with pathological facts, or with the results of physiological experiments.

The whole tenour of the evidence the writer has been able to collect, leads him to believe, that under no condition is the blood capable of directly exciting contraction of the muscular coats of the capillaries of any organ or tissue through which it may be circulating; that its influence on the blood-vessels can only be exercised, indirectly, through its effects on the brain and spinal cord (or, it may be, through the more purely local great sympathetic system); and that, as regards the arterial capillaries of any organ within the abdomen, the influence so exerted on them must eventually be of a nature the very opposite of spasm, as has already been so frequently explained.

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There is, moreover, really no evidence to show that, apart from its action on the cerebro-spinal nerve-centres, the blood in circulation through other organs and tissues possesses any power of action purely inherent in itself. There is, on the contrary, much reason for believing that it regulates itself simply by the laws of vital osmosis or dialysis. The writer uses the term "vital" here, because it would. appear as if a living membrane exercised greater power of causing transformations in the constituents of fluids passing through it, than a dead one does; although there is, nevertheless, abundant evidence that this property still persists to a greater or less degree, in the human body for some time after death, as is exemplified by the growth of the hair and nails, and the rise of temperature that then takes place in certain diseases.

He believes, however, that the views as to the condition of the minute arteries of the kidneys which have been recently advanced by Sir William Gull, and Dr. Sutton, and which attribute the thickening observed there to the results of an excess of nutrition leading to an increased bulk in the tissues of, and about, the capillaries—causing, at first, increase of size, but ending, eventually, in their contraction, through that shrinking and condensation which all such semi-organized textures undergo when they become of a permanent nature

—are quite in accordance with the facts recorded in respect of the changes which those structures at times undergo.

The process by which, as the bulk of evidence tends to show, this arterial change is really brought about, cannot, therefore, be made to accord with the view which attributes its origin to the induction of spasm and consequent muscular hypertrophy in the arterial walls.

It begins, originally, with a relaxation of the arterial muscular coats induced by nerve-influence; it is advanced by the deposit of fibrinous matter consequent on excessive nutrition following on the first named condition; and it is completed, eventually, by the contraction of the new semi-organized tissue thrown out in the course of the second part of the process;—every one of those conditions being, however, not only unfavourable to muscular contraction and invigoration of the capillary walls, but, from first to last, either opposed to those conditions, or inductive of such others as would, in the first instance, embarrass, and, eventually, render impossible, that due response of their muscular coats to vaso-motor nerve-influences which is essential to the right performance of their normal functions.

What has just been stated, is, the writer cannot but think, the true explanation of the nature and

origin of all, or of the greater portion of the pathological changes which pass under the name of "Bright's disease." He believes, further, that a similar explanation may be brought to bear, with equal force, on the conditions of the liver known under the term cirrhosis, which are evidently of a nature similar to those met with in the kidneys; and probably owe their origin to a like (it may be, in some measure, to part of the same) exciting cause,—due allowance being made for that modification of form which the deposit may assume in the liver, as compared with what is seen in the kidneys, and which must necessarily result from difference of arrangement of the minute anatomical structures of the two organs.

The important fact to be borne in mind, however, in connexion with this matter, is the existence of strong evidence to show that so-called diseases of this class are simply exaggerations of ordinary nutrition falling much short of the inflammatory process—to some form of which they have, nevertheless, hitherto, been generally attributed. It ought, perhaps, to be added here, however, that such deposit will always afford a basis on which other changes may readily be grafted in a way that might, at first sight, appear to invalidate what has just been stated.

As to the views maintaining that a direct influence is exerted by the blood itself upon the state of the renal capillaries, which it has just been attempted to combat, the writer would beg leave still further to remark that he cannot but regard them as part of that grand series of pathological errors which have, for years past, directed workers in this field of inquiry into altogether a wrong track; which in cholera attributed to the capillaries of the lungs, a condition which actual observation has proved does not exist; which accounted for the bowel symptoms of that disease by supposing the presence of a material cause, which most carefully conducted researches have proved cannot truly be said to have any real existence, inasmuch as they are in no way peculiar to that complaint; and which cannot, therefore, in the face of the physiological and pathological knowledge we now possess, be accepted as explanatory of the phenomena in question, in total disregard of a due consideration of the laws by which the vital processes of the human body are known to be regulated.

Much importance has very naturally been attached to the right explanation of the loss of muscular tissue which attends all febrile diseases, and to the relations which those changes, and the opposite state of increase of muscular bulk,

hold to the conditions of the circulation, and to the employment of active bodily exercise.

There are three conditions, both in health and in disease, which the muscles that are under control of the will may exhibit, namely, that of rest—voluntary in health, and constrained in disease; that of voluntary activity; and that of involuntary contraction.

Following out the views and principles which the writer has been attempting gradually to develope in the course of his essay, he will now endeavour to set down the lessons which the conditions just enumerated, when so considered, teach; not only in regard to the increase and waste of muscular tissue in particular; but also in respect of similar changes affecting the tissues of the body generally, both in health and in disease.

Reasoning from all we know of the conditions of nutrition and waste ordinarily existing in the healthy, full-grown, human body, we have reason to infer, that, when placed as much as possible in a state of quiescence as to voluntary motion,—the supply of a due proportion of food being kept up—it will, for a time at least, maintain its weight; and to expect that the functions of nutrition will go on uniformly and equally—no one part gaining at the expense of, or in advance of another.

A certain degree of equilibrium in the con-

stituents of the blood will thus be maintained—the fresh materials added as the result of assimilation of food, balancing those withdrawn either for the maintenance of tissues; to supply secretions essential to vital processes; or to be excreted from the body as matters effete and no longer useful to the economy.

By the transformations of the elements of the blood necessary to carry on the ordinary processes of life, what is understood to be the normal temperature of the body is maintained.

It becomes necessary, however, to remark here, that, although observers are at one as to the completed results of those transformations as they have reference to the production and maintenance of the temperature of the body generally, there is still apparently much difference of opinion both as to the manner in which the changes essential to the process take place, and in regard to the localities of the bodily structures in which they occur.

As regards the mode in which heat is produced within the body, the theory which has met with most universal acceptance, for many years past, is that which considers it to be a process equivalent to that of combustion, and which is dependent for the evolution of heat on the combination which occurs, chiefly in the lungs, between the oxygen of the air inspired, and the

carbon met with there as a consequence of the transformation of the elements of fatty, saccharine, and starchy matters contained in the blood.

By others, it would still be explained much in the same way, but with the addition gained by calling in the aid of nervous influence, which, by leading—through a supposed direct action on the constituents of tissues; and more especially as regards the muscular ones when in a state of voluntary activity—to immediate structural transformations, involves chemical changes and the evolution of heat inseparable from those.

This class of observers would also include with the foregoing changes, decompositions which they suppose take place in the blood whilst in the course of circulation, and which they believe to be independent of the processes of nutrition, secretion, or absorption; and, so far as their views can be understood, they would, still further, also, attribute to nerve-structures the power, in some way inherent in them, of actually generating heat.

There can, it seems to the writer, be very little doubt that the truth in this matter, as in most others that fall under dispute, lies, in some measure, midway between the different causes assigned; and that both the chemical view, and that which supposes an active display of the influence of the nervous system to be necessary for the initiation

and completion of the process, are concerned, although in a manner somewhat different from that stated above, in supplying the right explanation of the evolution of animal heat.

The grand error, however, which would appear to have been committed in conducting this investigation, and which has impeded advance in our knowledge regarding it, has, the writer would venture to say, been that of attempting to draw a too sharply defined line between the parts taken, respectively, in contributing towards the production of animal heat, by the non-azotized or so-called calorific, and the azotized or nutritive portions of the products of assimilation.

The employment of the term "combustion," as applied to the process which leads to the generation of animal heat, has done harm in another way by leading to the supposition that the production of heat within the body has any real similarity to that process which comes under our notice in every-day life, inasmuch as it must necessarily suppose, on that view of the matter, the essential and indispensable feature in the generation of animal heat to be, in some fashion or another, the union of carbon with oxygen—just as happens when the carbon of the coals of a common fire combines with the oxygen of the air.

There can be no doubt that much of the heat

generated in the human body is produced—especially in the lungs—by the union of the two elements just named; but the fact seems to be, that every act of nutrition requires for its completion, either a thorough or a partial transformation of some element of the blood, or constituent of the texture implicated. And although both those changes involve the production of that heat which contributes to maintain the temperature of the body, yet, even the degree of this, which the higher of the two forms indicates, falls far short of displaying such an amount of chemical change as would legitimately bring the process within the scope of what the term "combustive destruction" includes. In fact, the writer believes the use of this term to be altogether inappropriate to the explanation of the changes which take place within the human body coincident with the evolution of heat.

The action which is set up when sulphuric acid is added to water, and which causes heat to be evolved owing to fresh chemical combinations, is much more analogous, the writer cannot but think, to the changes which ordinarily attend the evolution of heat within the body, than the process of combustion is. The changes which do actually take place within the system connected with this process would really appear to be limited to such

as are consequent on solution, and on aqueous interchanges in the blood between its crystalloidal and colloidal elements; to sub-chemical vital transformations of the proximate elements of the blood, and of the bodily textures, in the processes of nutrition; and, also, to chemical re-arrangements of the ultimate elements of the blood in processes connected with the functions of the lungs, and with those of secretion and excretion—all which changes are attended with the evolution of heat.

Under ordinary circumstances, the absorption into the circulation of freshly assimilated matter, and the loss sustained in the course of the due performance of nutrition, secretion, and excretion, as nearly as possible balance each other in maintaining the equilibrium of the blood constituents.

This would appear to have been very conclusively established by Professor Parkes's experiments; and the transformations—chemical and other—which are essential to the completion of the processes above alluded to, as well as of those more immediately concerned in sanguification, and in the changes effected in the blood in its passage through the lungs—all of which are the results of various acts of vital osmosis, and of the conditions which those originate—are the changes on which the maintenance of the ordinary temperature of the body essentially depends.

There is, as has already been stated, no reason for believing that the completed materials of any secretion ever exist ready formed in the blood prior to the act of osmosis, concerned in the process of their separation from the latter; neither is there any good ground for considering that tissue changes are ever set up, through nervous influence, prior to the completion of the reverse act of osmosis which restores the changed tissue constituents back to the blood whence they originally came.

On the contrary, so far as can be seen, the evidence we possess tends just the other way, and would serve to demonstrate that changes of this description are originated and completed coincident with, and as the essence of, the commencement, progress, and conclusion of the act of osmosis on which the whole process depends—that having been fully perfected only when the material in process of removal and conversion has arrived on the side of the dialyzing membrane opposite to that from which it has been removed.

For those reasons, therefore, any marked production of heat will not take place on the side of the membrane from whence the elements of a tissue are being removed, but on that where the process is completed—a view as to the nature of the sequence of events connected with the origin of animal heat, in respect more especially to tissue

changes, which is, however, unfortunately, opposed to that which has at the present time gained considerable currency.

In other words, the rise of temperature, whether it accompanies the deposit or removal of tissue; or attends the process of secretion, or excretion; or is associated with that of absorption of matter in course of assimilation from the intestinal canal, is always coincident with the completion of the act of osmosis through means of which the entire process has been carried out.

The best proofs of the truth of the propositions which the writer has just advanced are to be found, perhaps,—in regard to those processes which entail the passage of materials from the blood-vessels outwards, as in the case of secretion,—in the results of the experiments instituted by Kühne relative to the generation of heat in connexion with the secretion of saliva—the observer in question having found the temperature of that fluid to be, when first formed, from 1° to 2° higher than that of the arterial blood supplied to the salivary gland.

In respect, also, to the generation of heat that accompanies the passage of matters constituting blood materials into the blood-vessels, the facts experimentally established in regard to the high temperature attained by the blood in the portal

and hepatic veins after digestion, are not less conclusive—it having been found that the blood in the portal vein, is, under those circumstances, several degrees higher than that of the neighbouring arteries; whilst that of the blood in the hepatic vein which results from further changes undergone by the blood of the portal vein in its passage through the hepatic structural arrangements intervening between the two veins just named, is, as might have been anticipated would result from the double heating processes it had been subjected to in such rapid succession, the highest displayed by the blood in any part of the body.

In accordance, therefore, with the principles enunciated above as to the manner in which the processes connected with organic life within the human body are carried on, the course of events which would follow on the supply of assimilated nourishment to the blood falling—from whatever cause—short of that necessary to maintain the equilibrium of its constituents against the various sources of loss to which that is liable, would be the setting of the osmotic current from the texture in which the required albuminous material exists, in the least stable form, towards the blood; and it would appear, moreover, that, as a rule, the muscular tissue at rest, is that which most readily responds to this demand.

This explanation of the mode in which tissue becomes broken up, and weight is lost, in health, from defective nourishment, is equally applicable to the like processes which may be so rapidly set up in disease, not only from deficient nourishment owing to defective assimilative power, but, also, through the loss of tissue necessary to meet the inordinate demand on the albuminous constituents of the blood created by abnormally increased functional activity originated in abdominal secreting organs—and notably in the kidneys —by unnatural intensity of cerebro-spinal nerve-influence reacting on the splanchnics.

It is, of course, equally applicable also to all changes of a like description that happen in the course of cholera, as it is to those occurring in fever, on the view that those two diseases are simply different degrees of intensity of the same process.

On no physiological subject of equal importance does there appear to be a greater want of precision of knowledge, in regard to the intimate textural changes and mechanism of action which attend its functional activity, than may be said to exist respecting voluntary muscular motion.

As has already been stated, on no texture of the body does diseased action in febrile disorders so readily exert its influence as on that of the

muscular system; and for that reason, and in the hope that some faint light may possibly be thrown on a matter so intimately bound up with the nature of the diseases of which this essay treats, the present chapter will close with a few remarks bearing on the subject alluded to in the preceding paragraph, and on its relations to the changes induced in muscular tissue during the febrile state—points on which it must necessarily be of the first importance to possess definite information.

The influences which regulate muscular nutrition and muscular action (for in the fulfilment of this, as of every other bodily function, those two conditions of nutrition and action are indissolubly united) would appear to be of a somewhat complex nature; and the only hope that seems to exist of any effort to throw light on the nature of the various conditions affecting them proving reasonably successful, appears to lie in the institution of comparisons between the various phenomena attending the processes in question, as they are usually carried on in health, and those occurring during the prolonged and involuntary contraction of the muscles of the trunk and extremities which are witnessed in the collapse of cholera, in one form of disease; or, perhaps, also, in the case of those which accompany the more violent, but

intermittent, muscular action seen in another—namely, tetanus.

The great fact to be noted in connexion with voluntary muscular action in health, is that of its being invariably attended with increased rapidity of circulation in and about the muscle being exercised; and, consequently, of its displaying all the phenomena which never fail to accompany that, namely, augmented nutrition and a rise of temperature of the part—the condition named keeping pace with the duration and amount of the exercise to which the muscle, or group of muscles, is subjected—subsiding, invariably, when the muscle returns to a state of rest; recommencing, with equal certainty, when its active employment is again resumed.

Knowing, as we now so thoroughly do, the full meaning of an increase of the temperature of any individual part of the body, we can readily ascertain for ourselves what is going on there, by noting the changes as regards temperature that it undergoes within a given time, coincident with such outward evidence as may exist of the exercise of its special functional action.

In a sufficiently ready, but somewhat rough way, any one accustomed to use dumb-bells just before stepping into his bath, may satisfy himself on this point by comparing the temperature of the

skin over the muscles of the upper arm and pectoral regions with that of other parts of the body, before and after some ten minutes' smart exercise.

It will thus be found how marked the rise has been immediately over the muscles most actively employed.

The increase of temperature is, at first, of a purely local nature; but when the exercise is sufficiently prolonged, the general heat of the body becomes considerably augmented by the natural diffusion of it, which must necessarily follow the circulation of blood of which the temperature has been raised by passing through the vessels of muscular tissues in which, from their employment, the nutritive process had been especially active.

This same point as to increase of temperature locally, was worked out, with the precision usual to that observer, many years ago, by the late Dr. John Davy, of the Army Medical Department; and the records of temperature which he then made are to be found in a series of memoirs on the subject, in the Philosophical Transactions, beginning as far back as the year 1814, and since re-published in a separate form. Dr. Davy has there shown, very clearly, that the parts under active muscular exercise usually undergo an increase of temperature amounting to several degrees.

In the voluntary muscular actions of health, the nerve-influence which causes muscular contraction evidently induces, or at all events is coincident with, inhibition of the vaso-motor nerves of the blood-vessels of the active muscle in a way that makes the action of the latter, and the increased nutrition which attends it, simultaneous, —the two states beginning and ending in concert, and being, under regular exercise, exactly proportioned to each other;—and, with a due supply of bodily nourishment, the muscular tissue so employed increases in bulk up to a certain point. This would appear to be determined by the limit to which the containing portion of the muscular fibre is capable of being distended by the fluid plasma in which the essence of muscle seems to lie, and on the increase or decrease of which the conditions of muscular strength or weakness would appear to depend.

The muscle-plasma being, from the nature of its constituents and its degree of fluidity, always in a state which renders it readily removable into the circulation, seems to possess all the characteristics which would render it the readiest source of reserve albuminous materials from which the general wants of the body could be, for a time, provided, when, from any cause, its regular supply of fresh pabulum has come to an end.

The view that seems to have, at the present time, gained currency as to the mode in which the nourishment of muscular tissue under active employment in the ordinary conditions of health, is carried on, is to the effect that increase of size takes place in the intervals of action, during repose—it being maintained, that, whilst actual contraction is going on, there is destruction of muscular tissue and loss of bulk—muscular action being, in its essence, held to be of a destructive nature.

The writer has, however, he regrets to say, after having to the best of his ability gone over the evidence available on the matter, failed to discover any reasonably good grounds for this belief. On the contrary, he would even venture to affirm his conviction that the bearings of all the facts accumulated on the subject, point in an exactly opposite direction.

Professor Parkes's invaluable researches show, that, during the continuance of active exercise in health, the amount of urea and uric acid excreted by the kidneys is diminished; and that it is not till after the muscular system has been allowed to return to a state of repose, that increased nitrogenous elimination is set up.

The only legitimate inference that can be drawn from this (knowing, as we do, that during the action of the muscle the greatest nutritive

activity prevails, as is manifested by the state of the local circulation and the rise of temperature of the part), is, that during the activity of muscular tissue, every available portion of albuminous matter in the blood is being used to increase the muscle-plasma; and that, during the succeeding state of repose, when, so to speak, the special conditions essential to muscular action have been thrown out of gear, and the circulation within the muscular tissues has again come to take its place simply as a part of that of the general organism, the extra reserve of albuminous materials accumulated during the period of increased nutrition which attended muscular action, is again drawn upon to meet demands which may arise in distant organs, or to restore the equilibrium in the constituents of the blood which the previous forced removal from it of albuminous matter may have disturbed.

There seems, further, to exist a strange general belief that the muscular tissue must undergo some peculiar change before it can be considered ready to yield up its constituents to the blood. All physiological observations on the subject seem, however, to prove that the part of muscle which does actually suffer loss, namely, its plasma, is always present in the same fluid state—varying certainly as to quantity, but ever ready to be

acted on by osmosis; and whether that may be by receiving fresh additions from the blood, or by yielding up its constituents to that fluid.

There are the strongest grounds for believing, that in every part of the body but one law regulates the nutrition of textures, and determines the intimate nature of the connexion that exists between the processes of nutrition, and of organic or textural functional activity in all parts of it; and there is no reason for inferring, that, —leaving out of question differences of initiatory nerve-influences, the consideration of which does not form part of the present argument—the nutrition and functional activity of muscular tissue are regulated on principles which are in any way exceptional to this general rule.

The second description of muscular action to which attention has now to be drawn, is that condition of spurious and painfully prolonged contraction of voluntary muscles, which, so far as the will is concerned, is incontrollable, and which, unlike the voluntary action of health, is invariably attended with contraction of the blood-vessels and consequent restriction of the circulation; with diminution of nutrition; and with reduction of temperature of all the parts more immediately concerned, including that of the skin in their vicinity.

The stages of collapse in cholera, and of rigor

in the more severe forms of ague, give the best examples of this species of action; and one striking feature connected with such cases is, that, when collapse proves fatal, a very remarkable rise of temperature occurs after death.

Allied to this is the condition witnessed in tetanus, where, however, as the state of the muscles, and the attending conditions of the circulation and temperature are of an intermitting character, the phenomenon of reduced bodily temperature is less marked than in cholera and ague. Nevertheless, bearing in mind the occurrence of those remissions and the modifying influence which they must exert, the very remarkable manner in which the temperature of the body rises towards the close of this disease, and the unusual height it generally attains after death, are sufficient reasons for considering the conditions of muscular action and of lowered temperature during life, in all three sets of complaints, to be, in some respects, allied in their nature.

What then are the inferences to be deduced from a full consideration and comparison, in all their bearings, of the different conditions just enumerated as occurring in health and in disease, in connexion with the functional activity of what are usually called voluntary muscles?

They amount simply to this, that a careful

review of the whole matter has been the means of exciting in the writer's mind very grave doubts as to the accuracy of the views at present held by physiologists in regard to the nature of the phenomena which lead to this description of muscular contraction.

If any point in physiology may be held to have been fully established, it is that which connects increased circulation, nutrition, &c., in any organ or texture of the body, with inhibition of the vaso-motor nerves of the blood-vessels of the part, and the consequent dilatation of the latter through temporary suspension of the contractile powers of their muscular coats; and which associates the opposite state of diminished blood-flow, and all to which that leads, with a constriction of blood-vessels owing to an opposite condition of their vaso-motor nerves.

In the case of voluntary muscular action, we have just seen that there is the strongest reason for believing, that, coincident, and it may be said, co-existent with the active employment of voluntary muscles, there occurs a state of inhibition of the local vaso-motor nerves leading to increased rapidity of circulation, &c.

In accordance with the views which, as at present entertained, hold the act of contraction of muscular fibre to be one consequent on directly-

exciting nerve-influence—the opposite of inhibition—it would become necessary to suppose that two sets of nerve-actions, diametrically opposed to each other, are occurring in apparently the same set of nerves, at one and the same moment,—namely, a direct excitory action as regards the fibres of muscles; and one of inhibition in respect to the muscular coats of the blood-vessels.

In the case of the action observed to occur both in regard to the voluntary muscles, and in respect of the blood-vessels which supply their textures under conditions of disease, as exemplified by the phenomena of collapse in cholera, and of severe rigor in ague, it is tolerably evident that the nerve-action going on with reference to the muscular fibres of voluntary muscles, and to those of the muscular coats of the arteries, if not actually identical in both instances, is so closely allied in character as to be virtually the same in regard to the influence it exerts on those two sets of textures.

The two typical conditions connected with muscular activity just named—that seen in the course of the ordinary voluntary action of health, and that which manifests itself in the stage of collapse of cholera—appear to be utterly irreconcilable and inconsistent with each other, when an attempt is made to explain their phenomena

according to the views at present held with regard to the nature of muscular action.

The writer would, however, now endeavour to advance a new and simple view of the matter, which, if found correct, will, he hopes, go far to remove the difficulties which at present stand in the way of reconciling all the known facts, without in any way doing violence to certain physiological laws which have now become pretty clearly established.

The exact nature of the minute structures of striated muscle has not yet been fully mastered by microscopists. This much, however, seems clear, that the fasciculi of fibres of muscle can be separated into minute, transversely striated, hollow, polygonal-shaped fibres, filled with fluid muscle-plasma; that in addition to the wall-proper of the fibre, which may be likened to that of a capillary blood-vessel, there is an outer fibrous sheath known as the sarcolemma, on which, according to some observers, both the blood-vessels and nerves are distributed, and into which, according to others, the nerves penetrate to reach the muscle-fibre proper.

Each fibre is marked off into innumerable transverse disk-shaped spaces of a lighter colour, by the intervention, between every two, of one of the narrow dark-shaded bands which form the characteristic striæ of each fibre.

The lighter coloured disk-shaped portions are capable of undergoing elongation in their long diameters by extension transversely to the axis of the muscular fibre; and when this happens, it is attended with a corresponding narrowing of the space in the direction of the axis of the fibre,—this narrowing causing the approximation of the dark bands on either side of it, and consequently inducing a shortening and darkening of that portion of the fibre, whilst its width is increased. Beyond being brought into closer approximation from the narrowing of the lighter coloured disks, the darker bands would appear to undergo but little change of size relatively to what happens to the lighter coloured portions, which, at the margin of the fibre, when in a state of contraction, project in minute bullæ considerably beyond the dark sections —the clear disks thus appearing to considerably exceed those latter in their length transversely to the long axis of the muscular fibre.

If, therefore, the tubular shaped muscular fibres in any way resemble, in their construction and endowments, the walls of arterial capillaries—and the readiness with which they respond to nerve-influence makes it more than probable that their clear portions are endowed with contractile properties identical with those of the muscular coats of blood-vessels—their darker portion being the

equivalent of the yellow elastic tissue coat of the latter,—only distributed in bands instead of in the form of a continuous structure,—then, by supposing that the dark bands are thus made up of comparatively unyielding fibres— and their observed action quite bears out this view of their nature—it becomes easy to understand how the mechanism of muscular action may be dependent upon the process of inhibition, instead of being the result of a directly exciting nerve-impulse, and may in this way owe its action to that same inhibitory influence which we know really does affect the blood-vessels of the part coincident with the occurrence of its muscular contractions.

The conditions in which the walls of a blood-vessel would appear to exist, vary between states of contraction; of dilatation; and of an intermediate, and as it might be called, normal one of tonicity. If the muscular fibre tube is endowed with properties similar to those of a blood-vessel, interrupted, however, at short intervals by the deeper coloured transverse striæ which are not possessed of contractile properties at all, or possess them in a much less marked degree than the clear portions, then inhibition of the nerves of the clearer disks of the fibre-walls would at once cause dilatation of those portions. Thei rinstant relaxation thus brought about, and the sudden dilatation

to which that would lead, could not fail to be attended with a lateral movement of their contained muscle-plasma. This motion once set up, would—according to the laws of hydraulics—gain in force, when, by the fibre-cylinder wall reaching its limit of dilatation, the lateral flow was suddenly checked. It would thus exert a distending power on the cylinder walls, and cause an extension of their clearer and more yielding portions in a direction transverse to their long axis, whilst leading, at the same time, to their narrowing in the opposite direction—thus bringing the more unyielding dark bands, on either side, into close proximity, and producing shortening of the fibre with increase of its width.

By a simultaneous action of every disk of the fibre-cylinder, and of all the fibres of each fasciculus of a muscle, an immense amount of hydraulic power, comprised within materials contained in a very small compass, would thus be excited.

This would act with a vigour proportioned to the degree of tension existing in the fibre-cylinders which would bear a relation to the amount of plasma they contained, and would, consequently, be in proportion to the degree of increased nutrition of the muscles and neighbouring parts resulting from the augmented rapidity of circulation invariably attendant on voluntary muscular action.

On the cessation of the inhibition of its nerves,

the fibre-cylinder would, as a natural result of the tonicity of its walls, regain the dimensions usual to it in a state of repose, and the whole muscle would return to its ordinary degree of elongation—the process being repeated with every recurrence of contraction.

On this explanation, the one inhibitory nerve-action would account not only for the state of increased rapidity of the circulation in the part, but would, at the same time, make it possible to expound the mechanism of muscular contraction, and the mode of its initiation, on a principle which the writer cannot but believe to be rational in conception, and not only in accordance with our knowledge of the teachings of science, but in harmony, also, with that unity of plan in regard to the regulation of her processes, from which there is such strong evidence to prove that nature so rarely departs.

But, further, once the accuracy of the foregoing views as to the nature of voluntary muscular action and its attending physiological phenomena witnessed in health, can be admitted, the explanation of the condition noticed in the stages of collapse in cholera, and of rigor in ague, becomes readily and consistently attainable, and affords, moreover, in one respect, evidence confirmatory of the accuracy of the views above recorded.

What, then, is the condition into which, under active cerebro-spinal nerve-influence associated with manifestations of disease, the voluntary muscles, their blood-vessels, and those of the surface of the body in their vicinity are thrown, which forms so striking a contrast, in cholera and ague, to the actions set up in those textures in the course of the execution of the ordinary function of voluntary muscular action in health?

There does not appear to remain the slightest ground for doubting that the condition of the parts just enumerated, which exists, at the surface of the body, simultaneously with a state of inhibition of the great sympathetic system of nerves within it, is one of direct irritation (the opposite of the state known as inhibition) proceeding from the great cerebro-spinal centres; causing contraction of the muscular coats of the capillary blood-vessels; inducing painful involuntary constriction (without normal contraction) of the muscular fibre-cylinders; and thus producing a corded, knotted, or hardened condition of certain muscles of the trunk and extremities in the more marked cases of ague, and in cholera;—conditions which persist until, with the setting in of reaction, relief comes, simultaneously, to both the external and internal parts of the body, by the abatement of the grand central nerve-influence which had originated and

was maintaining the whole series of unnatural states of functional action now under consideration.

The evidence that the state of the blood-vessels at the surface of the body and extremities is caused by persistent contraction of their muscular coats is so clear, and has already been so often dwelt upon, that it will be unnecessary to again do more than barely allude to the fact of its existence.

In regard to the co-existing condition of the fibres of voluntary muscles, however, it will be necessary to remark that the state into which they are thrown being a painfully, and persistently, involuntary one, and, at the same time, one incapable of producing action similar to that usual to those muscles when performing their normal functions in health, cannot, therefore, be considered due to a form of action in any way equivalent to that which is set up under the influence of the will.

The view as to the nature of this state of the muscles which is suggested by the supposition that the explanation of the mechanism of voluntary muscular action which the writer has advanced is the correct one, would go far to demonstrate that all the unnatural conditions of voluntary muscles enumerated above as occurring in cholera and ague, are consequent on a

persistent contraction of the contractile portions of the muscular fibre-cylinders, of a nature similar to that which is going on, at the same time, in the neighbouring blood-vessels; and, moreover, whilst affording a rational explanation of the phenomena in question, its adoption would, further, attain this end in a manner strictly consistent with all that has already been brought forward in connexion with this highly important subject.

The shrunken tissues and low temperature—followed, after death, by a very remarkable increase of the bodily heat—show, very conclusively, that, as regards the blood-vessels, the constriction of their walls which persisted during life has, at last, given way to relaxation. As regards the muscular tissue, also, the proofs that the condition under which that suffered was, during life, one of constriction of the walls of its fibre-cylinders, and that, like the parallel condition of the walls of the blood-vessels, this state, too, has likewise given place to one of relaxation, can, the writer believes, be no less clearly proved by a consideration of the phenomena of the automatic movements of the limbs that have been so frequently seen in patients just dead of cholera in its stage of collapse.

Had the cramps of cholera been occasioned by persistent *bonâ fide* muscular contraction

occurring after the fashion in which the mechanism of that process is understood at the present day (not as the writer has attempted to explain its nature), the withdrawal of the direct exciting nerve-influence on which it depended, could only have caused immediate relaxation of the muscular fibres, and flaccidity of the limbs, which would have lasted until *rigor mortis* set in. According, however, to the writer's view of the mechanism of muscular contraction, the immediate result of the removal of this nerve-influence would be the sudden relaxation of the walls of the muscular fibre-cylinders, and the production of an act of muscular contraction in all respects resembling that occurring in voluntary motion; and which would last until what remained of the natural elasticity of the fibre-wall brought it back to a state somewhat short of the extreme degree of forced dilatation, which the hydraulic force excited in the muscle-plasma by the first effect of relaxation of the fibre-walls would naturally have induced.

On the old view of muscular contraction, the mechanism of those automatic movements cannot be consistently explained without the assumption that fresh nerve-force is generated after death.

All our information on this point would tend, on the other hand, to show, that no such vital

force as that which the exercise of active nerve-influence would imply, ever occurs after death—such actions as do take place being confined to those of a more purely physical character, including a certain amount of nutrition and growth of peculiar tissues, and various degrees of chemical transformation.

In the "gastric variety" of yellow fever also, in which death occurs under circumstances closely analogous to those met with in the stage of collapse of cholera, as shown by a cold and shrunken surface; a fluttering, failing pulse; and by the intellect remaining clear to the last;—but with this difference, however, that the bowel discharges in the former disease are voided upwards by the mouth, in the form of profuse black vomit, owing, there can be little doubt, to the presence of intense nerve-inhibition, which, although not altogether confined to that part of the intestinal canal, falls —for some reason, probably connected with the special seat of origin of the regulating cerebro-spinal nerve-influence which originates it—with peculiar severity on the vaso-motor nerves of the œsophagus, stomach, and upper part of the intestinal tube;—to repeat, in this variety of yellow fever there is a similar rise of temperature; and automatic movements of a like nature to those met with in cholera, have been frequently observed

to occur after death. The explanation of this latter phenomenon, is, of course, the same as that which has already been offered in regard to the movements observed after death from cholera; and the *post-mortem* rise of temperature is to be accounted for in the same manner in both instances.

The fact of the conditions just referred to having been met with in yellow fever, is, however, of still further importance in enabling us to connect this disease with those others to the consideration of which this essay has been specially devoted; and to identify it as being, to all appearance, merely a variety of non-contagious fever (of a degree of intensity only, perhaps, falling somewhat short of cholera) which probably owes its origin to especially peculiar conditions of climate, and of terrestrial and electro-magnetic influences; and, perhaps, to ways of living and modes of employment, which cannot fail to be widely different, in the West India Islands, from the parallel class of conditions met with in India.

But it may be asked, what analogy can there possibly be between the clear coloured characteristic discharges of cholera, and the black vomit of yellow fever? The answer is, that beyond the existence of the dark tinge communicated to it by the presence of blood blackened by the acids of the gastric secretions, the real fluid of black

vomit would appear, by all accounts, to be of a similar watery description to that of cholera,—with this difference, however, that, for a reason which will be readily apparent, the one is acid and the other alkaline.

Moreover, in a considerable proportion of cases, the discharges of cholera, at first colourless like the fluid brought up preliminary to the setting in of black vomit, become, towards the close, tinged with blood. In this case, however, the colour is pink, owing to the absence of darkening acids from the intestinal fluids.

It is impossible to speak with any amount of certainty of a disease of which he has only seen two spurious examples, but, judging from the excellent accounts of it given by Drs. Blair and Davy, the writer cannot help remarking how much he has been struck by, all things considered, the close parallelism that exists between their descriptions of yellow fever and the leading features of the various degrees of non-contagious fever met with in India. Dr. Blair himself admits that the three forms into which yellow fever has been divided,—namely, the "simplex," the "mitior," and the "gravior,"—are really but stages of the same form of disease;—cut short at its first stage, like the corresponding cases, so often returned as febricula or simple continued fever in this country,

which do not advance beyond a single paroxysm of ague ;—ending within a day or so, when there has followed on the reactive stage of the first paroxysm a state of congestion of the surface attended with slight extravasation of blood under the conjunctiva and skin—the effused blood gradually changing, as blood so effused usually does, to the yellow hue to which the disease owes its name ;—and, lastly, there is the third variety, which, when it proves fatal, would appear to do so in the stage which is evidently a species of collapse forming the cold stage of an incipient severe exacerbation of the disease, in which marked inhibition of a special section of the internal sympathetic leads to the black vomit, and to failure of the heart's force, whilst the coincident constriction of the external vessels causes the cold and shrivelled skin—the patient dying, with all his faculties unclouded, before reaction, which would have disturbed them, has ever set in.

So much has already been said on this part of the subject, that it does not now seem necessary to remark on the kindred phenomena of tetanus further than to notice, that, although in this disease the abnormal state of the muscles is intermitting, and there is evidently mixed up with it much of the condition of real muscular contraction—using this term in contra-distinction to what is meant

to be expressed when speaking of the state of the muscles seen in cholera—yet, taking all things into consideration, the conditions which affect the temperature during life and cause its rise after death, are, in many respects, parallel in this disease and in cholera.

CHAPTER IV.

THERAPEUTICAL AND HYGIENIC.

The writer would preface this part of his subject by at once saying that the treatment which has, he cannot but feel, proved eminently successful in his hands in the management of non-contagious fevers as they are met with in the tropics, has been that which may be called, *par excellence*, the quinine treatment pure and simple.

More than twenty years ago, the writer had the good fortune to be introduced to tropical fever and its treatment by quinine, when serving in Ceylon, as an assistant surgeon under the orders of Dr. J. C. Cameron, now Deputy Inspector-General of Hospitals on half-pay.

That able medical officer's experience in the treatment of this description of fever had been very extended; and the evidence of the success of the plan which he followed was so marked, as to have, in a very short time, convinced the writer of its practical value. Experience has now taught him, empirically, certain additional details and modes of procedure that have helped to make (as he hopes to be able to demonstrate) the quinine

treatment as successful in his hands as it is, perhaps, well possible for any human means to become —so far at all events as the actual saving of life is concerned; but it has only been within as many months as his first knowledge of the general plan has existed for years, that there began to come to him a dawning of the scientific grounds on which the faith that has been in him for all that time rested.

The writer will now endeavour, in the first place, to record, as briefly as possible, the mode of treatment which he has finally adopted in tropical fevers, and to state the results which have followed its employment.

He will then, in the second place, attempt to lay down a system of treatment for cholera, which, as it is founded on the rational principles he has endeavoured, in an earlier part of this essay, to enunciate as to the nature of that disease, and is, moreover, based on the success which has undoubtedly attended the employment of measures devised on similar principles and directed against what he believes to be simply milder degrees of expression of the same disease, he cannot but feel hopeful that the adoption of the procedure he is about to recommend, may be found to be attended with more favourable results than have, as yet, crowned any of the numerous attempts made to combat this hitherto extremely fatal malady.

Although, as has already been stated, when serving in Ceylon between the beginning of 1851, and the corresponding period of 1855, the writer had learned the good effects which attended the employment of a pure quinine treatment as applied to the fevers of that island, and he then had considerable experience of its power in cutting short that disease, it was not until he landed in India towards the close of 1858, and had been brought face to face with the treatment of fever on a large scale, that he was able to properly estimate its full power over that complaint, and thoroughly to appreciate the value which attached to its employment at the very outset of the disease, if its power in arresting fever was to be to the fullest extent brought out.

At Azimghur, when in medical charge of H.M.'s 6th Foot, from January to the end of August, 1859; at Meean Meer in the Punjab, when he was Surgeon of the 51st Light Infantry, from the middle of September of that year till February, 1861; and again, when serving with the Royal Artillery, at Lucknow, during the hot season of 1869, he had very extensive experience of the treatment of tropical fever, and that, too, always with the same unfailing results;—namely, that, not only among numerous cases that were treated by him on the one uniform plan now to be

detailed, did no death occur, but in not one single instance did the disease run on to the confirmed remittent or continued types properly so called, although, at the same time, in the same station, and even in a portion of the same hospital—but under a different plan of treatment—cases of fever assumed not merely the more severe forms of the complaint, but even, in some instances, ended fatally.

It may be here stated, parenthetically, that in 1860, whilst not one fatal case of fever took place in the hospital of the 51st Light Infantry, at Meean Meer, two occurred in the 79th Highlanders occupying parts of the same barracks and hospital; three in the 51st Detachment, under the senior Assistant Surgeon, at Umritsur; and one at Fort Lahore—in addition to the manifestation of many severe and prolonged attacks of the disease among all the troops referred to, with the exception of those under the writer's own immediate care.

It was not, as has been found to have been the case in England, when the advantages of a so-called "restorative treatment" in regard to pneumonia were being demonstrated, that fatal cases were not reckoned when the post-mortem evidence disclosed the existence of any other coincident morbid condition. It is positively the fact that no deaths whatever, from non-contagious fever—

complicated with other diseases, or otherwise—have ever occurred in the writer's practice.

In hospitals under his own personal charge, the writer has seen only three examples of the more severe forms of fever:—The first he found in the hospital of the 51st Regiment, at Meean Meer, on assuming medical charge of that corps in September, 1859,—there having occurred, between the 1st April and the beginning of September of that year, no fewer than eight deaths from fever in that same hospital previous to the date of the writer's assuming charge of it; the second was sent in to the head-quarter hospital from a detachment at Fort Lahore, and when admitted it was found that, under the old form of treatment, the case had assumed the severe continued form of the disease; the third instance occurred in the same hospital, in the case of a soldier treated throughout the attack by the senior Assistant Surgeon of the corps, whose mode of managing fever was altogether different from that uniformly followed in all cases under the writer's immediate care.

To those three cases, therefore, and to the experience which he gained whilst visiting, in consultation, the patients of other medical officers in the same Cantonment, the writer's acquaintance with the more severe forms of fever has been confined.

He must here repeat, however, that the knowledge he gradually acquired as to the most successful mode of treating fever was attained entirely on empirical principles, whilst his ideas as to the nature of the different forms of the disease were then crude in the extreme. Such as they were, however, they early led him, in practice, to disavow the existence of simple continued fever as an independent disease; to doubt that typhoid or enteric fever existed in India as a form of complaint apart from so-called remittent fever; and they induced him to include the fevers he met with during the cold season, as a rule, under the term intermittent, whilst he returned those of the hot season chiefly under the head of remittent fever.

He would add, further, that although ever since the year 1861, the subject of fever, and the relations borne to each other by the different forms which that disease assumes, have more or less occupied the writer's attention, it was not until after 1869, when he had the good fortune to hold chief medical charge of the Lucknow Circle during a severe epidemic of cholera, that the connexion between that disease and fever began to dawn upon his mind; and it is only within the last two years that he has been able to think out the subject in such a manner as he would humbly trust

may, in some measure, be found to contribute to the satisfactory unravelling of, and, in some degree, providing a remedy for, what has hitherto proved to be not only an unfathomable mystery as regards its nature, but has also resisted every plan of cure which up to the present time has been brought to bear upon it.

The reasoning on which his views are based has been fully set forth in the preceding part of this essay, and the writer will now endeavour to confine himself to simply enumerating the remedial measures he has found so effectual against fever, and to afterwards detailing the modification of their employment, which, reasoning from analogy, he would fain venture to hope will be found to be not devoid of results when directed against that higher form in which fever expresses itself, which the present writer believes constitutes cholera.

The one fact which it is of primary practical importance to know in connexion with the treatment of fever is this,—that, as a general rule, the disease, as it is met with, at all events among well-cared-for troops, and when seen early, is to a certain extent highly amenable to any form of judicious treatment which, at the outset, includes rest and purgation, and closes with the exhibition of quinine.

Under treatment based on this old established system, however, the cures are not of a permanent nature, relapses are frequent, and a very considerable proportion of the cases run on to the more dangerous forms of the complaint. It is, moreover, unfortunately, impossible at the beginning of the attack to determine what case will not end in the latter way, whilst it must ever be remembered that once the more truly remittent or continued symptoms have fairly declared themselves, the occurrence of bowel complications, and the mortality from those, or from cerebral affections, will, among such cases, be large, whatever plan of treatment may be adopted.

The writer came, consequently, early to understand that the only really effectual means of contending against the more violent forms which fever so often assumes, were those measures to which he could look with certainty to altogether prevent their occurrence, and which consisted in the adoption, at the very outset of the disease, of one uniform system of treatment,—however mild the attack might in the first instance have been.

The mode of procedure adopted by the majority of medical officers in the management of fever in this country, so far as has come within the writer's knowledge, is that of employing the so-called ordinary febrifuge measures as a prelude to the

exhibition of quinine, which it seems to be the rule to believe should not be administered till the force of the disease has been broken.

It is this preliminary delay at the very outset of the disease, and at the only stage of it when the appropriate remedy possesses the power of acting with really telling effect, which, the writer cannot but think, proves fatal to the chances of an attack being cut short by proper treatment.

Once such an amount of unchecked reaction has been set up as, by its violence or repetition, to lead to effusion of serum within the head, the whole character of the disease becomes changed, quinine exhibits but little influence over its progress, and the prospect of recovery, after at best a prolonged illness, becomes very much a question of strength of constitution, of the use of counter-irritants, of careful nursing, and the judicious employment of restorative measures—final recovery leaving the subject of the attack with a constitution severely shaken, and it may be, in a state of body rendering him but little fitted for further service in the tropics.

The plan of treatment to which the writer would now beg to call attention is as follows:—

On admission (and as a rule, soldiers who become the subjects of fever in India are sent to hospital either in the course of the preliminary

stage of malaise, in that of rigor, or, at latest, at the outset of the reaction which follows the initial occurrence of the last-named condition), if diarrhœa does not exist, a purge consisting of a drachm of compound jalap powder with two grains of calomel, to which five grains of quinine have been added, is at once administered, the patient is ordered to bed, and from six, to eight or twelve leeches are applied to the temples,—the bleeding being at once arrested the moment the whole of the leeches have dropped off.

Cold is then applied to the head, and lemonade and tea are allowed for drink.

The five grain doses of quinine are repeated thrice daily until all symptoms of fever have disappeared. The dose is then reduced to three grains given three times daily—which quantity is continued until the patient is discharged to duty, to which, in the 51st Regiment, he invariably returned on the second day after leaving hospital—leave for the first having been granted as an indulgence by the Commanding Officer to enable the man to clean and arrange his accoutrements and necessaries. In that way the pernicious system of sending men out of hospital before they have quite recovered from their ailments, and granting them "convalescent leave" in barracks, was done away with, to the great advantage of the

health of the corps, although at the risk of causing an unfavourable contrast to exist between the daily list of sick in hospital in the head-quarters of the 51st, and that of the 79th Regiment quartered in the same barracks, as well as that of the 51st Detachment at the neighbouring station of Umritsur, in both of which the plan of treatment pursued was of a totally different nature.

On his discharge from hospital, each fever-patient is provided with a supply of quinine pills, which he is directed to take, every night at bed-time, to the extent of five grains of the remedy, for the space of one week after his return to barracks.

When on admission, as frequently happens, diarrhœa is present, the same quinine and other treatment is at once commenced, but without the purge. In such cases, however, small quantities of Dover's powder (3 to 5 grains) are given with each dose of quinine.

Should there occur at the outset of reaction—and this is common in the more severe cases met with at the commencement of the hot season—intense headache with much irritability of stomach—the vomiting being generally a symptom of more than usual cerebral congestion, as the more violent pains of the loins and limbs are of a like condition of the spinal cord—leeches are

employed in larger numbers, and their application is repeated if necessary.

Even in such cases, the administration of five-grain doses of quinine is, with the aid of effervescing draughts, at once commenced, and, if diarrhœa is not present, the bowels are acted upon by purgative enemata. In all instances of fever of this nature attended with irritability of stomach, the writer would now improve upon his former practice by administering quinine hypodermically.

With regard to this mode of employing the drug, he must, however, state here, once for all, that he would strictly confine its use to the cases in which it was found to be impossible to administer the remedy by the mouth, and for the following reasons: —in the first place, because in ordinary cases of fever when there is no irritability of stomach, this little operation is unnecessary; and in the second, because, although they are of rare occurrence, accidents have followed its use to an extent quite sufficient to render it at least a matter of prudence to reserve its employment for cases where quinine cannot be effectually introduced into the system in the ordinary way. Under any circumstances, the writer would strongly recommend that the locality selected for the puncture should be the skin of the trunk—say at, or a little below, the base of the axilla—and not that of the arm or

leg where it is impossible to altogether avoid the hurtful effects which unavoidable muscular action is always liable to exert on even the slight local irritation, which, at the best, must, under the circumstances, follow the injection of a somewhat acrid fluid.

The writer would here add, that he has fully tried the effects of quinine when given in doses larger than five grains. He has, however, given up using the drug in that way, having found by experience that better results follow the employment of the five-grain doses, which rarely cause cinchonism—a condition, the production of which has, he has seen reason to believe, rather an injurious effect than otherwise on the progress of the disease. On that account alone, he considers it advisable that the employment of the larger doses should be avoided, leaving altogether out of question the unnecessary waste thus involved of an invaluably useful, and proportionately expensive, remedy.

Whilst making this statement, however, he would beg it to be distinctly understood that he in no way wishes to impugn the accuracy of the views of those who, under different circumstances of disease, and especially in their practice among the natives of India, have seen good results attend the use of quinine in much larger doses.

As has been stated above, when the disease has already run on to its more severe forms, quinine, under any circumstances, exerts but little influence for good on the disease, and its exhibition in five-grain doses may even be positively hurtful.

The writer has, however, in his limited practice in such cases, always continued to give the drug to the extent of nine grains daily, and has usually persisted in its exhibition for some consideralbe time, not only after the complete subsidence of the urgent symptoms, but for a longer period than usual during and subsequent to convalescence.

As regards the accidents known under the names sometimes of heat apoplexy, sometimes of sunstroke, concerning which the writer has, in a previous part of this essay, expressed his belief that they are simply cases of excessive cerebral congestion occurring chiefly—though not always—during unusually hot and oppressive weather, either in the course of the first onset of reaction, or during an exacerbation, later on, in an attack of fever, it would only be reasonable to expect that, at all events, the essential part of the treatment which has most influence in arresting fever should also have some beneficial effect in relieving its complications. Accordingly, the hypodermic injection of quinine (the only way in which this remedy can be employed in such cases) has

already been tried, in India, in many such cases; and it is stated that a certain degree of success has attended its employment.

This plan of treatment, however, so far as it has been employed, has been adopted on altogether empirical principles, and in no respect, so far as the writer is aware, from any conclusive recognition of the existence of a connexion between the condition in question and ordinary fever.

The administration of quinine should undoubtedly always be tried, in such cases, along with the other means usually employed; but as in those accidents the disease has already advanced to a stage beyond that one in ague during which the influence exerted over the disease by this special remedy is most decided, the writer cannot hold out, on his view of the nature of those attacks, any very strong hopes that the amount of success to be expected from this plan of treatment will be very marked, although, at the same time, he fully expects that it will probably so much exceed in usefulness any other mode of procedure hitherto tried, as to fully justify, and even to demand, its steady employment.

Heat apoplexy, however, the writer believes, can only be properly met by its entire prevention (so far as that may be possible) through the exercise of more than usual vigilance over the

health of the troops, and the adoption at such times and under such circumstances as the attacks in question are most to be looked for in, of means to ensure the early and effectual treatment in the way he has recommended, of every case of fever, be its symptoms, at the outset, ever so mild.

In order to meet the wish which will probably be felt by most of his readers, to be provided with a few details of a more specific nature as to the actual results of the plan of treatment the adoption of which he has so especially recommended for fever, the writer would beg leave to submit the following particulars, in regard to this point, which he happens to have preserved.

Between the 1st April and the 31st December, 1859, the average strength of the 51st Light Infantry, at Meean Meer in the Punjab, was 639; there were 1,078 admissions for fever; and 8 cases of that disease proved fatal.

The writer assumed medical charge of the corps on the 10th September of that year, and he then found under treatment in hospital an example of remittent fever in its severe form, but which eventually recovered after a long illness. From the date given above, till the close of the year—a period which includes some of the worst fever

months in the Punjab—there was not only no death from fever at head-quarters, but no case of that disease assumed a severe form.

Between the 1st January and the 31st December, 1860, the average strength of the corps, the head-quarters of which continued at Meean Meer, was 526, and the total cases of fever amounted to 169. Two of these, however, which have already been referred to as having, under a mode of treatment different from that adopted by the writer, fallen into the more severe form of fever, and which were under treatment for 18 and 49 days respectively, have been eliminated from the total 169 in estimating the average residence in hospital.

Out of the whole number admitted, no deaths occurred, and no case treated by the writer assumed a severe form. The average period of residence in hospital (every man having been detained there till perfectly fit for immediate duty) of the 167 cases treated specially on the writer's plan, was 7·58 days.

At the detachment of the corps stationed at Umritsur from the 1st January to the 12th November of the same year, when it rejoined head-quarters, out of a strength of about 500, there were 139 admissions for fever, of which three died —the disease terminating fatally on the 8th, 48th, and 7th days of the disease respectively.

The average period of residence (including that of the three fatal cases, and with a system of indefinite convalescent leave in barracks which probably greatly shortened the time spent in hospital) was 12·67 days.

This, as has already been stated, happened under a system of treatment altogether different from that pursued by the writer.

The 79th Highlanders occupied parts of the same barracks and hospital with the 51st Regiment, at Meean Meer, in 1860. The average strength of that corps was 953, the admissions for fevers were 177, and the deaths two; whilst several more cases assumed the severe form of the disease. The writer has no means of estimating the average period of residence in hospital.

At the neighbouring citadel of Lahore, a detachment of one company gave 49 admissions and one death from fever, besides sending an example of the severe remittent form to head-quarters.

As has before been mentioned, the writer's experience in the management of tropical fevers in the East, conducted him step by step, but in empirical fashion, to the adoption of the completed plan of treatment which he has above recorded, and regarding which he would almost venture to hope that, in the face of the details he has been able to give regarding the results which have attended

his employment of it, it will not be considered presumptuous in him to affirm that, however it may fare in the hands of others, it has in his own proved as successful as it is well possible for the treatment of disease ever to become.

Although the writer was fortunate in starting well, in this respect, on the occasion of his first doing duty in the tropics, yet the knowledge in regard to the treatment of fever, which he believes he now possesses, was not attained at once. Old prejudices, of which he became from time to time aware in regard to what is still considered by the majority of medical officers to be the only correct mode of administering quinine, having had to be considered, and, after experience of their worthlessness, totally disregarded.

In connexion with this subject it is not only curious to note, but also of importance to know, as a means of demonstrating the truth of what the writer has already more than hinted at,—namely, his belief that, virtually, not only are all fevers in the eastern tropics merely modifications of each other, and of the corresponding fevers met with in temperate climates, but that the fevers of both the East and West Indies are simply local modifications of the same types of disease—yellow fever in the West Indies, taking the place, as the highest expression of fever usually

occurring there, which cholera holds, in India and elsewhere, as the highest manifestation of the corresponding diseases of those countries.

It is moreover, further, he cannot but consider, of the utmost importance to recognise the fact that some of the most intelligent army medical officers and most trustworthy observers, have, in the West Indies, passed through a course of educating-experience as to the fevers of those regions, their relations to each other, and the way in which they may be most decidedly influenced by the use of quinine, almost identical with that which, in India, has fallen to the lot of the present writer;—thus demonstrating, in a very remarkable manner, the wisdom displayed by the earlier medical authorities in giving so much weight to the consideration of the action of the "*juvantia* and *lædentia*" as a means of identifying the relations, and contributing to explain the nature, of apparently different diseases.

In the Appendix to the third edition of Dr. Blair's account of the yellow fever epidemic of British Guiana, edited by the late Inspector-General, Dr. John Davy,—and which is by far the best monograph of the disease that has yet been written,—there is to be found evidence of what has just been stated, in memoranda on the quinine treatment of fever as practised by them

in the West Indies, furnished by now Inspectors-General of Hospitals Drs. T. Ross Jameson and J. Miller, and from which the writer will take the liberty of making a few extracts.

In a communication dated 8th December, 1849, and addressed to Dr. Davy, Dr. Jameson, after stating that a few days after landing in Jamaica in 1834, he saw, under the superintendence of an old medical officer, a few sporadic cases of fever marked by yellow suffusion, and terminating in black vomit, adds that he was informed by this gentleman that intermittents were rarely witnessed in military practice, but that, when they did occur, they were readily cured on the anti-periodic principle—a plan of treatment, however, his informant added, which was not followed in the more severe forms of fever—quinine being, in such instances, only administered as a tonic during their apyretic convalescence.

At page 148, he then remarks:—"3. At a very early stage of my investigation, it appeared to me that the *endemic* fever, as it exists in the *Lowlands*, was erroneously termed remittent; that it was indeed, strictly speaking, an intermittent, inasmuch as, always within forty-eight, and generally within twenty-four hours, there appeared a most distinct apyrexial period.

"4. From the above opinions it naturally

occurred to me, that during this apyrexial period quinine should be administered: that, in fact, in these fevers the *anti-periodic* treatment was clearly indicated.

"As the season advanced, and those cases occasionally merged, *by imperceptible degrees*, into well-marked remittents, I soon found this medicine could be safely administered *during febrile action*, providing such action, however *absolutely* severe, was yet in itself the sequence of a severe pyrexia,—was therefore, in fact, relatively an abatement or remission.

"The practice then adopted was to discover in all cases the moment when the paroxysm should have reached its height,—for whenever this climax was fairly passed, quinine was commenced without further delay, and its exhibition continued during the abatement, and during the remission or intermission, as the case might be. When the fever returned, the medicine was no longer given.

"5. In 1835 I began to doubt if I had pushed the administration of quinine to its legitimate extent, for the following reason:—

"I had clearly demonstrated that it might be given, within certain limits, during the presence of pyrexial action: that, in fact, the old established treatment of intermittents—styled anti-

periodic—was equally applicable to all the *severer types of Jamaica fever*. Yet this practice, when fully carried out, involved occasionally a somewhat paradoxical treatment. For example, if called to two cases, each presenting the *same apparent* symptoms, it sometimes happened that, in the course of investigation, the one was found *relatively* to be in the stage of abatement progressing towards remission:—whilst in the other, the paroxysmal severity was on the *increase*. In the former case, quinine was administered; in the latter, it was withheld.

"On suggesting that in such cases the pyrexial action, however *relatively unequal*, was yet *absolutely* of the *same intensity*, and that therefore this medicine might be given in both, the idea was at once repelled by an old experienced surgeon, who remarked, 'Give quinine during the paroxysm! why it should never be given when *fever is present at all:* when the bowels are open it is most useful in apyrexia, and during convalescence, but then *only*.'

"6. Whilst treating severe cases during 1837, I again thought my anti-periodic plan, however superior to the old modes, was in itself defective, as it left untouched the *initial paroxysm;* and in all the more malignant fevers observed during that year, it appeared to me, that this paroxysm

alone, when uncontrolled, caused organic lesions sufficient to destroy life ultimately.

"7. It was not until the spring of 1839, however, that I met with a case, of which the previous history, as I then thought, might justify me in exhibiting this medicine *throughout the paroxysm*, according to my new proposal.

"The case I now allude to was as follows :— A young gentleman who had not then completed his eighteenth year, early in 1839 was attacked with the usual seasoning fever, at Up-Park. It was sufficiently severe to leave the result for some time doubtful. During convalescence he went to Stony Hill, a station believed to be nearly 1,200 feet above the sea level. In a few weeks afterwards he came to Kingston, and was there attacked with tertian intermittent. Although the type was simple, the seizure was very severe. He was treated on the old anti-periodic system,—recovered,—and went to duty. In a few weeks he again relapsed, this time it was a *double-tertian ;* he was similarly treated, and became convalescent. The season was advancing,—there was, therefore, every probability that if he had another attack, it would prove most dangerous. I accordingly determined, if such an event should occur, to adopt my new plan, viz., to exhibit—in combination with such other remedies as the case might indicate—

quinine continuously, without regard to the presence or absence of the febrile paroxysm, from the moment I was called in till the issue of the case was apparent. The fourth attack soon came (July). He had not been two hours ill when I saw him, but the symptoms were well-marked and most violent. I at once administered, with other agents, 20 grains of quinine, and continued the medicine till, at the end of eight hours, he had taken 80 grains. In about twelve hours from the first accession, the fever disappeared, and up to the period of my departure from Jamaica, in 1845, it had never returned.

"8. During the summer and autumn, I treated a great number of cases of the ordinary *endemic* with remarkable success. In the December of the same year, I happened to read, in a colonial newspaper, that fever had just appeared epidemically at New Orleans, and that in some cases it had been checked by this medicine at the moment of accession. The dose was 40 grains. This statement confirmed my resolution. Till then I had not deemed it possible to *cut short* a case of *epidemic* fever by any remedial agent whatever.

"9. In Jamaica, fever could not be said to prevail epidemically from the end of 1837 up to the winter of 1840. It then assumed a most

malignant epidemic form, which continued for about fifteen months.

"In February, 1841, when at Spanish Town, I made my first trial, and a successful one, in a case of this epidemic fever which occurred in civil practice. I continued my plan during March and April: in May, when quartered at Fort Augusta, a soldier's wife recovered very remarkably. On my return to Kingston early in June, I persevered, and indeed, *since that period*, all patients under my sole care have been treated on this principle *exclusively*.

"10. During the month of May, a somewhat similar mode of treatment was adopted at Port Royal by Senior Surgeon Williams, of the Royal Artillery, and in September following this gentleman tried a modification in the application of the new principle, which I had once followed.

"In the majority of cases the bowels are not confined, and in some even diarrhœa is observed. In those, the plan adopted was—to exhibit immediately a full dose of quinine and calomel, and to continue this prescription, regardless of the pyrexia. In many instances, however, especially in plethoric Europeans, the bowels *were* constipated. In these, a smart purgative was exhibited in the first instance, and then the quinine. This officer judiciously added the quinine to the *purgative*

itself, and thereby in such cases gained, on an average, at least two hours, a great matter in a disease so rapidly fatal.

"11. The evident success of the early and continuous exhibition of this remedy in all fevers caused attention, and many practitioners—both military and civil—adopted it. It has generally been grafted on some of the old systems, and, as far as I have been able to learn, by no one who tried this treatment has it ever been relinquished."

Again, at page 152, Dr. Jameson remarks:—

"15. From the *days of Hunter* up to the present time, the *characteristic feature* of all the Lowland fevers of Jamaica has always been the pathological phenomenon styled *periodicity*. This periodicity is to be regarded as the direct effect of the local poison, popularly termed miasm; and, of all remedial agents yet discovered, the most powerful in combating the morbid effects of this virus appears to be—quinine."

The writer was not, until quite recently, aware of the existence of the memorandum from which he has just been quoting; and it is, he cannot but believe, a matter of no small importance, in relation to the question of the identity of the fevers of the Old and New World, that so close an analogy should exist between the views at which

two observers have arrived, quite independently of each other, in regard to the most effectual mode of treating those diseases.

In a letter dated 18th March, 1843, addressed by Dr. Miller to Dr Davy, the following remarks on the administration of quinine in remittent fever occur. They are noticed here as showing that gentleman's belief in the utility of the early administration of quinine, even without the employment of any preparatory measures.

The observations referred to, occur at page 157, and are as follows :—"The administering of purgative medicine as a preparatory step does not appear necessary. It was at one time thought that quinine would act more rapidly and in less quantity by first evacuating the bowels freely. Several cases have come under my observation where the quinine was given at once, and with an equally good and certain effect; and, where the symptoms were urgent, I would not now be disposed to wait for the operation of purgative medicines. Calomel has not been found necessary, and was rarely given except as a purgative."

And again, at page 158, he remarks :—

"The curative powers of quinine in the remittent fevers of this colony can scarcely admit of a doubt. When properly administered the duration of the disease ought not to be allowed

to extend beyond twenty-four or thirty-six hours."

The quotations given above speak for themselves in a way that renders unnecessary any further notice by the present writer.

Before proceeding to treat of the remedial measures to be specially directed against cholera, the writer has only to remark, in regard to the employment of local bloodletting in cases of fever, that, although he believes that this measure is but seldom resorted to in the practice of others, and he is aware that a large majority of cases recover without the application of leeches to the temples having been resorted to at all, yet he is convinced that the employment of this measure is a source of considerable relief to that cerebral congestion which, together with a like condition of the spinal cord, it has already been pointed out forms one of the essential features which characterise the outset of fever. He feels, moreover, pretty well assured that this measure when combined with the proper administration of quinine, is a most valuable auxiliary towards ensuring the two conditions most to be desired in the management of fever,—namely, the cutting short of an attack at its outset, and the removal of that tendency to relapse which, in ineffectually treated fever, is so liable to occur, and to become not only damaging

to health, but even dangerous to life, by the complications which persistence or repetition of the complaint is so apt to induce.

It now only remains for the writer, under the therapeutical division of this part of his subject, to detail a plan of cure to be tried in cholera, to the development of which all that has gone before has been leading up, and from the adoption of which—should there be any truth in the reasoning he has hitherto been employing; and unless, up to the present time, he has been altogether deceived in regard to the estimate he has formed of the value of the essential points of this particular system of treatment when directed against the milder forms of the same genus of disease—of which he holds that cholera forms only the highest degree—there can scarcely fail to flow results of a practical importance which will prove to be, he would venture to hope, by no means contemptible.

Reasoning from the principles on which the writer has endeavoured to explain the nature of cholera, any mode of treatment which may reasonably be expected to prove efficacious when employed against that disease, must naturally divide itself into two stages—the disease being so intensely marked in the symptoms present in each of the two grand divisions (collapse and reaction) into which it naturally divides itself, as to call

for the adoption of more special measures, to meet that excess of action, than are necessary for the less intense shades of fever.

All the really essential points in the treatment to be adopted must, therefore, divide themselves into :—

First,—measures that are specially directed against the preliminary stage (if that can be recognised), and that of collapse into which this passes ; and *second*,—expedients more particularly intended to moderate and control the succeeding stage of reaction—that now usually known as the consecutive fever of cholera.

The subsequent stages of subsidence of reaction, and of convalescence, as they become almost, if not altogether, identical with the corresponding ones of ordinary fever—each in its degree—and require a similar course of treatment, will scarcely require to be further remarked upon.

1. As regards the first of the sections into which the disease has, in respect of the manner in which curative measures are to be employed, been divided, it will, of course, be of the utmost importance that the commencement of the attack be recognised at the earliest possible period ; and, as it has been clearly established that, as in ague, there is a preliminary stage of that complaint, of from twelve to twenty-four hours' duration, which

occurs in advance of the initial rigor, so, in cholera, it has been found that there is a period of departure from health, prior to the setting in of collapse,—the highest expression of rigor,—which is of a like continuance.

At the outset of an epidemic of cholera, the preliminary stage will, as a matter of course, be but rarely recognised, as it will not at first have been looked for; but, so soon as the general prevalence of the disease has declared itself, careful attention to the condition of bodies of men, or of portions of a community, will soon be the means of leading to the detection of the earliest appreciable indications of approaching attacks.

As the principle on which the writer's plan rests is that of the earliest and fullest possible employment of quinine; and as in cholera, not only is it impossible during the stage of collapse to retain medicines given by the mouth—and even if they could be retained, we know, from the condition of the blood-vessels of the intestines which then exists, that they could not be absorbed into the system whilst that continues—the only effectual way of administering this remedy will be that which hypodermic injection supplies.

The first point, therefore, essential to the success of the plan of treatment about to be proposed will be the administration of quinine, by injection

under the skin, at the earliest possible period at which either the active stage of the disease, or the signs indicating its approach, can be recognised. The first step, therefore, which the writer would beg to insist upon being taken without a moment's delay, is the injection of the equivalent of ten grains of quinine, to which has been added one fourth of a grain of the acetate or hydro-chlorate of morphia, either in a single operation on one side of the trunk of the body—say at or below the base of the axilla—or, in two quantities on opposite sides, but introduced in as quick succession as possible.

A mustard-plaster, made powerfully active by the use of strong vinegar, and of considerable width, should then be applied along the spine from the nape of the neck to the lower part of the lumbar region. When this has been done, a compress of folded flannel, or of woollen cloth, and of a size sufficient to cover that region, should be applied to the abdomen, and retained by a flannel or other roller, or by a binder of any description, applied as tightly as can be borne.

Frictions by the hand, or with soft warmed cloths, should be made to the cramped limbs; and drinks *ad libitum* of weak tea, toast water, rice water, milk and water, thin gruel, lemonade, or soda water, and either cold or hot as may suit the

patient's feelings, should be given frequently, in small quantities at a time, and persevered with in spite of vomiting. Considering the nature of the process that is going on within the body—however that apparent to the senses on its surface may contradict this—the writer would, by preference, recommend the use of cold drinks, and the swallowing of small lumps of ice, as the measures most in accordance with the real nature of the condition existing within the body during the persistence of collapse.

On no account whatever should wine or any other form of stimulating drink be allowed.

The patient should be kept strictly to the recumbent posture, and there should be no attempt to restrain the bowel-discharges, although every effort should be made, by suitable arrangements, to ensure their escape in the posture of the body least likely to cause fatigue or induce exhaustion.

In one hour from the time the first was used, let a second injection of quinine be made, but this time to the extent of only five grains, and without the addition of morphia, which should only be employed once at the outset of the treatment.

The drinks and frictions should be continued as circumstances may render advisable; and when the mustard-plaster shall have produced its full effect, it should be removed and the abdominal pad,

and the means employed to keep that in its place, carefully re-adjusted. The administration of wine or stimulants should still be strictly refrained from.

Should the vomiting and purging not have ceased at the end of two hours from the time of the second injection (three hours from that of the first), a further quantity of quinine to the extent of five grains should be administered, and repeated if necessary, in the same way as on the previous occasions, and the other measures should be continued as may seem advisable.

Should vomiting, however, have quite ceased, a five-grain dose of quinine might in that case be given by the mouth, either in substance or solution, and repeated every third hour afterwards unless cinchonism shall have been produced.

Should that condition have supervened, the further exhibition of the remedy must be withheld until the state in question shall either have passed off, or have become less marked.

2. When the symptoms of collapse begin to show signs of abating, the patient should be very closely watched, and (the quinine being still, according to circumstances, continued or not as above recommended), when reaction has fairly commenced—for example, when the temperature of the axilla has risen to 99° or 100°—but before it has become fully developed, from a dozen, to

twenty leeches should be, without delay, applied to the temples; or—what would probably be far more efficacious and certain of action—the patient should be bled from the arm to the extent of six, eight, or ten ounces, according as the disease may have occurred in a woman or delicate soldier (the writer is now speaking of European troops and their families; but the mode of treatment, he is advocating, may be readily adapted by those who best understand their constitutions, for native troops, and for the natives of India generally), in a man of average strength, or in one of more robust build. Cold should at the same time be applied to the head, and the drinks continued as before; but stimulants in any form should still as much as ever be avoided.

3. Should the local or general bloodletting and the remedies already employed, have moderated reaction and restrained the tendency to intense congestion within the head, which is so fatal in cases of cholera that have run their course unrestrained by appropriate treatment; and should there be evidence, in the fall of the temperature in the axilla, and in the increased strength and diminished frequency of the pulse, that the febrile symptoms are gradually abating, the administration of quinine may be reduced in frequency to three doses daily—and, eventually, in quantity also,

to three grains given three times a-day. The latter dose should, however, be continued throughout, and even beyond, the full period of convalescence.

When reaction has completely subsided, arrow-root, beef-tea, jelly, and such like nourishment, with wine, may be administered; but until this stage of the disease has been reached, the employment of more than the ordinary fever-drinks can only prove hurtful.

4. When convalescence shall have fairly set in, food of a more solid description, together with a due allowance of wine or beer, may be permitted; but, as has already been recommended, the use of quinine in small doses should still be persevered with. Should, however, any special symptom arise in the course of this period, it ought to be met in the manner usually followed under the ordinary circumstances of such an occurrence; but in cholera, as in every other form of the genus of febrile disease now under consideration, as it has been clearly made out that the times when relapses are most liable to occur may be set down, with tolerable certainty, as being that of the close of periods in days, counting from the onset of the disease, which are multiples of seven—it may be, a little more or somewhat less—it seems essentially desirable that the administration of quinine should be continued, in every instance of

recovery from such complaints, so as to carry the convalescence under the protective influence of quinine beyond at least two of those periods,—counted from the outset of the disease as to their times of incidence, and from the cessation of active symptoms in respect of their numbers. This seems really to be the only true *rationale* of the power exerted by quinine in warding off relapses; and it is a principle, moreover, the due recognition of which may give a determinate direction to efforts of this nature, made through the employment of this invaluable remedy, which they have never yet possessed, and which can scarcely fail to give, also, a degree of precision to a highly important branch of therapeutics which will, in practice, be not only encouraging to medical men, but, it may be, fraught with no small ultimate benefit to mankind.

With regard to the special symptom usually looked upon as one of much evil omen—namely, suppression of urine, the writer has merely to remark that as this condition is simply the result of a loss of the watery portions of the blood and tissues, and although important as indicating the extent to which that deprivation has gone, it is not to be looked upon as evidence of anything untoward having happened to the renal functions or structures. The medical attendant should,

therefore, if necessary, re-assure his patient on the subject, and content himself, in regard to it, by waiting for the re-appearance of the absent secretion, which will, in due course, certainly follow when some degree of equilibrium between the blood and tissues as to the due relative amounts of their more watery parts has again been restored. This change can, however, only be effected after such an amount of fresh fluid shall have been taken into the body, and absorbed into the current of the circulation, as will be sufficient to facilitate the necessary re-adjustment of the relations above alluded to.

For the reasons already given, the patient should on no account be harrassed by the introduction of the catheter to ascertain whether the bladder contains urine—it will always be found empty ;—and no effort should be made, by the use of diuretics, to hurry the restoration of a process which can by no possibility be re-established until the sources from which it is derived shall have regained somewhat of their normal condition.

Should, unfortunately, alarming head symptoms set in during the stage of reaction, these, also, must be looked upon as being, simply, examples of intense cerebral congestion such as are so often met with in the corresponding stage of fever. They ought to be dealt with accordingly, and

should in no case be considered, as they hitherto have so often been, in the light of symptoms of uræmic poisoning—there being no reasonable ground for believing that they are in any respect of that nature.

It has been observed that recoveries are made from well-marked yellow fever, with a rapidity which is in remarkable contrast to the prolonged convalescence that follows severe attacks of the remittent or more continued forms of fever met with, at the same time, in the West Indies; and this well-known fact has been made use of as an additional argument in support of the view which holds that yellow fever is altogether a disease *sui generis*, and in no way connected with the more common fevers which are never absent from that part of the world.

The fact that, considering the alarming nature of the symptoms, recovery from cholera is also rapid, has attracted considerable notice.

Both the occurrences above referred to, however, the writer cannot but maintain, supply arguments of no mean power in support of the views and principles which he has been endeavouring to enunciate in the preceding part of this essay—his object having been, he would again repeat, to show that both yellow fever and cholera are but more highly expressed forms of ague presenting

intensely exaggerated cold stages, and which generally prove fatal during the persistence of that condition, although they also, in many instances, end fatally in the succeeding reactive stage.

Cases that do recover, however, return to a state of health in much the same way as is seen in examples of aborted ague—that is, after the accession of only one act of rigor (collapse), and of the subsequent state of reaction (consecutive fever) which follows on that.

The conditions of body which cholera and yellow fever leave behind them, although in striking contrast to those consequent on attacks of the severe remittent and continued forms of fever (the specially serious and unmanageable characteristics of which manifest themselves by becoming superadded to the early stage of pure ague in the way already fully explained in another part of this essay), show, however, not only from the great loss sustained of the watery constituents of the body, but also in consequence of the escape, along with those, of some amount of the more important elements of the blood,—as seen in the black vomit of yellow fever, and as observed, also, in respect of cholera, in the presence of blood corpuscles in the discharges of a considerable proportion of cases of the latter disease;—those conditions, it is repeated, show the existence of a decidedly lower state of

health in the two more serious forms of complaint than that which has to be recovered from after successfully treated cases of ague.

Nevertheless, the analogy that exists between the two sets of conditions, is, all things considered, so close, as to form, almost, the writer cannot but think, a climax to the cogency of the reasoning he has adduced in proof of that virtual identity of all the forms of non-contagious fever, which he has been at so much pains to endeavour to demonstrate.

So much labour has already been expended, and even, he fears, repetition inflicted, by the writer in his endeavours to make the principles he has been trying to establish clear, that it appears almost unnecessary to add any further explanation in order to show the grounds on which the curative measures recommended for employment in cholera have been based. But, lest any of his readers should still wish for further details, he will venture to subjoin a few additional explanatory sentences.

As it is now scarcely possible any longer to doubt that quinine, administered at the earliest possible moment after the onset of an attack of ague, has the power of cutting short that complaint, so, at the very outset of cholera (which is but the highest expression of ague), it is but reasonable to expect that, if any remedy can avail

in that disease, quinine, promptly employed, must be the one to be had recourse to with any degree of confidence.

As, however, the intensity of the disease is, from its outset, of the highest degree, the dose of quinine which the writer has recommended to be adopted in the first instance, is one which, considering its mode of administration, cannot fail to be, in its action, equivalent in power to three times the ordinary five-grain dose to which the writer has come to limit each administration of this remedy in cases of ordinary fever at their early stages. He has, moreover, recommended the subsequent employment of the remedy in question in moderate doses, repeated at short intervals, in preference to giving a larger quantity at one time.

Experience may, however, render it necessary to modify the mode of procedure now recommended.

The dose of morphia named (which, when administered hypodermically, may be considered equivalent to a full one given by the mouth), and which is to be added to the first dose of quinine, is given as an auxiliary to the latter, on the principle that, long before the use of bark in the treatment of ague had been superseded by that of quinine, the older physicians had discovered the efficacy of a full dose of opium, when employed in ague at the outset of rigor, in cutting short the latter condition

Probably, hydrate of chloral may prove equally, or, it may be, even more efficacious, if used at this stage in room of the opiate; but, as the writer has had no experience of the use of the newer remedy in any form of disease, he prefers to recommend the one for the employment of which he knows that such good warrant exists.

The use of counter-irritation over the spine is advised, in order, if possible, to relieve, by derivation, that degree of congestion from which the spinal nervous centres, in common with the cerebral ones, are known to suffer.

The object in view in recommending the application of a compress and tight roller to the abdomen is, by giving support, and applying compression, to the abdominal viscera, to assist, in some measure, to restrain the relaxation and distention of their blood-vessels which, there are such strong grounds for believing, result from inhibition of the abdominal sympathetic, and on which the alarming bowel-symptoms must depend.

Blood-letting is of course recommended in the hope that, if made use of, at the right time, in advance of the acme of reaction, it may have some influence in restraining the cerebral congestion about to supervene, whilst, at the same time, relieving that which already exists, both within the head and spinal canal, as the first appreciable

indication of the presence of the chain of sequences which has led up to the more marked symptoms of the disease—the good results which undoubtedly attend the early employment of this measure in ordinary fever, giving reason to hope that its adoption will also prove beneficial in this higher expression of that form of complaint.

The writer would here further add, that, with regard to the use of local blood-letting which he has recommended in ordinary fever, and which, in his own practice, he never fails to employ, the remedy in question is prescribed with a view to relieving that condition of congestion of the great cerebro-spinal centres, which, it is now known, forms one of the essential features of the outset of fever.

The writer is, moreover, convinced that the practice is an excellent one, and not only affords early relief to the patients' feelings in respect of headache and pains in the loins and limbs, but acts also as a very powerful auxiliary to the other means employed, in assisting to at once cut short the disease and prevent the occurrence of relapse.

As regards the other measures which follow, no explanation as to the object of their employment need be offered.

Under the second section into which the writer has divided the present chapter of his essay, it

will be desirable to inquire what bearings the views he has advanced on the subject of fever, and on the relationship borne to each other by various forms of disease, hitherto held to be independent as to their nature and origin, have on the prevention and mitigation of a class of complaints which, perhaps, in relation to human life in every part of the habitable globe, plays a part second in importance to that exercised by no other series of diseases.

At the very outset of such an investigation we are met by the all-important, and unavoidable consideration, that any views capable of bringing under one category, and demonstrating the existence of one common origin for what have, hitherto, been believed to be altogether distinct diseases—each acknowledging a cause specially peculiar to itself—must, if correct, vastly aid in bringing about a simplicity and directness of action in regard to all sanitary and hygienic measures employed for their mitigation, which could scarcely have been attained whilst such efforts were being projected under a belief as to the real nature of what was being contended against, which fell short of the actual truth to the degree implied by the propositions which the present writer has been so earnestly striving to maintain.

As he believes he has been able to adduce what he cannot but think will be found to be very

strong proofs that all the species of the genus of disease, which he has, for the sake of convenience, distinguished in the preceding part of this essay as non-contagious fever,—whether they may show themselves in the form of ague, at one end of the scale; as cholera, at the other; or in any of their degrees intermediate to those two extremes,—are, in their whole series, merely modifications of one and the same process, and dependent for the forms of expression they may assume, upon the varying degrees of intensity in which certain combinations of their exciting causes may, for the time being, and under ever-varying circumstances, exist, the writer trusts that, in the face of all this, he may be pardoned for entertaining a hope that it may be found that the data likely to ensure the attainment of the object referred to in the preceding paragraph, as one the realization of which is so earnestly to be desired, has already been, to no small extent, supplied in the preceding pages of this essay.

Before, however, any further progress can be made in this direction, it becomes advisable to consider two points:—first, whether any member of this special fever-series is really of a contagious nature; and, second, whether any one special cause exists to which, more than to any other, the origin of the whole, or of any one

species of the genus, may be attributed, and of which the complete removal, if that could be effected, might reasonably be expected to lead to the total disappearance of the diseases specially dependent upon it.

1. In regard to the point to be settled as to whether any of the group possess contagious properties, the writer would at once distinctly state that he has had the belief forced upon him by his own actual experience in the management of a cholera epidemic in India, and from a careful study of the facts that have been adduced in connexion with the investigation of this vexed question, that there is no real evidence to show that, under ordinary circumstances, any one of the series, from the highest to the lowest in the scale of intensity—and including cholera and yellow fever—possesses contagious qualities in the true meaning of that form of expression—the term "contagious" being, in this instance, used in the same sense as that in which it is employed when spoken of in relation to small-pox, measles, scarlet fever, or typhus, when it implies the power possessed by a disease of spreading either in consequence of vicinity alone without actual contact, or from both vicinity and contact—conditions which are, however, in reality, only different degrees of the same thing.

There has, at various times, been placed on record abundant proofs to demonstrate, that, on all occasions when cases either of cholera or of yellow fever have been removed from unhealthy houses and insanitary localities, in which, owing to a common exposure to one generally prevalent exciting cause, those diseases have affected individuals in a way which, at first sight, could only lead to the belief that the disease had spread from person to person through means of contagion :— to repeat, when such cases have been removed to properly regulated hospitals occupying comparatively healthy localities, experience, of no limited nature, has shown, that it may be looked upon as almost a certainty that not one of the attendants on such cases—however closely the nature of their charge may have brought them into contact with the sick, and even should their clothes and persons, at times, have become tainted (an accident of by no means rare occurrence in such circumstances) by the discharges of the sick—will be attacked by the disease. The writer has made use of the expression—almost a certainty—because, during the epidemic prevalence of such diseases, it never can be reckoned upon with absolute certainty, that, even in the most healthy locality of any place where such epidemic exists, persons in immediate charge of the sick may not suffer from the

universally prevalent exciting cause of whichever of the two complaints (cholera or yellow fever) may at the time be about, just as might have happened had they been employed outside the hospital in some other avocation.

Accordingly, to all who have in India had practical experience of the charge of troops in times of the epidemic prevalence of cholera, the recollection of instances will occur where a drunkard, or a weakly man, has been picked out by the prevalent disease from among the hospital staff, just as other men similarly conditioned have been singled out from among the companies of a corps —occurrences which, however, in respect of the hospital attendants, happen only in so sparse and occasional a way as to prove, on the whole, by the circumstances under which they present themselves, the accuracy of the general proposition which the writer has just advanced.

The histories of epidemics of cholera, both in India and in England, abound with examples of the immunity above referred to as being enjoyed by the attendants on cholera patients, provided always the latter have been removed from the localities in which the attacks may have originated, and have become disassociated from the collateral circumstances which favoured their occurrence.

It may, however, prove not uninteresting to his

readers if the present writer quotes the latest published record of such an instance happening in India, which has come under his notice whilst these pages are passing through the press: and the more so, as the paper from which it is taken was written with a view to prove cholera to be a truly contagious disease.

The incongruity of the reasoning employed by the writer of the paper in question, in his endeavours to square his arguments with the facts he has had to adduce, would appear to have very forcibly struck an acute non-professional observer, who, in a leading article in the *Indian Daily News* for the 9th January, 1873, devoted to a review of "an account of cholera in the Looshai force" contributed to the *Indian Annals of Medical Science* by Assistant Surgeon R. T. Lyons, of the Indian Medical Service, offers the following remarks, made with reference to the fact that it is clearly shown by Mr. Lyons' paper that the attacks of cholera had, as regarded the whole body of the force, occurred in a sparse and apparently capricious manner:—

"Now, considering that there was free intercourse between all classes of persons, and that no restraints were imposed on such intercourse, until people were actually stricken down, is the theory of internal predisposition to become vic-

tims to a universally, or generally, prevalent poison, at all more 'unintelligible,' than the theory of external contagion. Surely cholera poison 'shows,' to use Mr. Lyons' own words, 'a partiality, as it were, in its operations on human beings,' if the contagion for which he contends, selects victims in the most capricious manner.

"The present writer once held executive charge of a large jail in Bengal, into which there was a terrible invasion of cholera. Quarantine had been enforced in the most rigid manner, and yet the disease went on from man to man, picking out apparently doomed men from the most distant wards, and leaving all others unharmed. Segregation effected some diminution in the disease, but it was most successful when, with segregation, other influences operated to improve the physical tone of the prisoners. The Civil Surgeon in medical charge of the jail considered the origin of the disease a most mysterious one: the writer, after giving the matter such attention as a non-professional mind could give it, could only see in the progress of the disease itself so many proofs of the existence in individual men of physical conditions which made them peculiarly susceptible to the disease—no matter whence it came. But there are other grounds on which most persons will find it difficult to accept the theory of contagion without reserve

—some of them suggested by Mr. Lyons' own admissions."

In a continuation of his remarks on Mr. Lyons' paper, which follows in the next day's issue of the same newspaper, the writer of the article above quoted from, adds the following observation in regard to quarantine in jails—a knowledge of which cannot fail to be of vital importance to a right understanding of the true value of any evidence which has, up to the present time, been founded on jail experience respecting questions affecting epidemic diseases in relation to the presence or absence of contagious properties :—

"It is usual to talk of the rigid enforcement of quarantine in jails ; but the simple fact is that, in no jail in Bengal, has rigid quarantine ever been absolutely enforced. Even where the severest measures have been carefully carried out, the daily visits of the jail officer, the Civil Surgeon, and the jailor, all of whom have daily mingled with the outer world, may fairly be regarded as channels of contagion. So that, however rigorously quarantine may in all other respects have been enforced anywhere, it is impossible to affirm that the plea of the enforcement of quarantine is fatal, under the circumstances of the case, to the theory of contagion. All that can be fairly claimed of quarantine, as it has been enforced under the most

favourable circumstances, is that it has reduced the risk of contagion to a minimum; while, on the other hand, it must be admitted that considerable laxity has obtained in many jails in which quarantine has been supposed to have been enforced."

After mentioning a particular instance of special laxity in this respect which was the common practice in the Rajshahye jail, in 1868, the writer of the article adds :—

"It is difficult to say now how many of Dr. Mouat's most important statistics may not have been completely vitiated by similar laxity in the jail administration of other districts in Bengal."

The writer of this essay will now content himself by quoting a passage from Mr. Lyons' paper (the main object of which seems to be, as has already been stated, to prove that cholera is a contagious disease) noticed by the writer of the newspaper article at present being referred to, in order to show how strangely contradictory views on that subject may run, side by side, in the minds of medical men who are, nevertheless, firm believers in the thoroughly contagious nature of cholera—a frame of mind which, the present writer would here, parenthetically, remark, that the published proceedings of the International Sanitary Commission, held at Constantinople in 1866, show very clearly must have possessed the

members of that Commission in a very remarkable degree, and with a result to the world at large, he regrets to feel bound to believe, of which the influence for evil, in some respects, to the welfare of mankind, will probably never be fully appreciated.

The quotation to which the writer specially refers is taken from page 420 of the *Indian Annals of Medical Science*, and is as follows :—

"Besides the fact that no case of cholera could be unequivocally traced to contagion contracted from the sick in hospital, incidents transpired in the hospital which negatived the conclusion that the sick were capable of communicating the disease. It was found impracticable to prevent communication between the sick and the other patients. . . . . Observing all these infringements of sanitary rules in association with the fact that no case occurred among the attendants on the sick in hospital, it is difficult for me to come to any other conclusion but that cholera is not communicable by persons actually suffering from the disease." And again :—"The result of all the foregoing observations made regarding the behaviour of cholera is, that while cholera is undoubtedly contagious when it is present in a party of men, the sick are incapable of communicating the disease. The reconcilement or explanation of those two discrepant circumstances is a matter

of no small difficulty in the present state of knowledge regarding the disease."

As regards more especially the question of contagion in connexion with yellow fever, the careful researches by Dr. Blair on this subject, as recorded in the Appendix to his work on yellow fever already quoted, are as conclusively negative of that disease possessing the power of diffusing itself by contagion, as it is well possible for any evidence to be: and not only so, but the history which he gives of the progress of the special epidemic,—to a description of which his book is devoted,—would tend to show that the immunity from attacks of the disease enjoyed by the attendants on the sick in the hospitals into which yellow fever patients were admitted, was such as would go far even to prove not only the total absence of contagion, but to make it probable that those so employed were exempted from the disease to a degree which did not fall to the lot of those who, in probably less healthy situations outside the hospitals, went about their usual avocations, and were not brought into actual association with examples of the disease.

As it is with medical men who are brought at times, almost daily, and that, too, sometimes for years in succession, face to face with yellow fever in the West Indian, and neighbouring, British

dependencies, that the belief that the spread of the disease in question is not due to contagion, has, in the most striking manner, risen to conviction :—so, in India, it is with the medical officers who have had most practical and executive experience of cholera as it occurs among European troops, and who, of all those practising medicine in India, have the best opportunities of forming a correct judgment as to whether that disease also possesses contagious properties, that a similar belief in the negative may be said to be almost universal.

For many years past it has been maintained by a certain class of writers on diseases of tropical climates, that the first introduction of yellow fever into the West India islands came from distant countries in vessels from which cases of the disease had been landed ; and although the strongest proofs had been adduced to show the inaccuracy of those assertions, still, within quite a recent date, fresh examples of similar alleged importations from which fresh yellow fever epidemics are stated to have sprung, have been brought forward, and firmly maintained by writers engaged in England in the compilation of works on the practice of medicine.

Some remarks by Dr. Blair on the two most recent of such instances, seem to the writer to be so conclusive as to the untenable nature of the

evidence on which they have been accepted, and appear, at the same time, to possess such valuable bearings on the whole of the subject now under consideration, that he will make no apology for here giving an extract of the passages to which he refers. They occur in the Appendix to Dr. Blair's book already quoted, and will be found at pages 136 and 137. They are as follows :—

"From some observations on the origin of the endemic fever which recently occurred in the garrison of St. Ann, Barbados, and of which such frequent mention, as to certain particulars, has been made in the foot notes, I beg to refer to a paper on the subject published in the September number of the Edinburgh Medical and Surgical Journal, in which the hypothesis is advocated that the disease was of local origin, and neither imported nor contagious, contrary to a report that it was introduced by Her Majesty's steamer "Growler" from the Coast of Africa, that vessel having touched at Barbados in proceeding from Trinidad, which she first made, to Bermuda.

"Since that paper was written I have not ceased to make inquiry respecting the 'Growler' during the few days she was detained at Barbados, with the hope of obtaining the most exact information; but I cannot say, with success. The difficulty of learning the naked truth on occasions

of this kind is hardly credible to the inexperienced in such investigations.

"I may mention a few circumstances in illustration. The report that the 'Growler' introduced the fever, was first spread after the first appearance of the disease. Careful inquiry then made on the spot contradicted it, and of course satisfactorily proved it to be without foundation. . . . .

"Coincidence seems to be an inexhaustible source of error, and that almost of necessity, from the nature of cause and effect. Apart from the coincidence of the outbreak of the fever partially in the garrison of St. Ann, and the arrival of the 'Growler' with fever on board, no one circumstance favoured the conclusion that it was thus introduced. The two fevers, I think it may be asserted, were of a different kind. No fever was introduced into Port of Spain, Trinidad, by the 'Growler;' none into Bridgetown, Barbados; none into the family with which the sick when landed in Bermuda freely communicated; and further, for many months, the fever afflicting a part of the garrison of St. Ann, was confined to that portion of the troops stationed in the low grounds where it first broke out.

"Coincidence by itself surely is of no account; it is constancy which gives it importance in the relation of cause and effect. In obscure cases, it

should be only suggestive. The late fever in Bona Vista, the first appearance of which was coincident with the arrival of the 'Eclair,' with fever on board, from the Coast of Africa, by many has been considered as a demonstrative example of the importation of the disease: yet Dr. King, who was employed officially to investigate the matter, arrived at the conclusion that it was of local origin; as, in his opinion, was also of local origin that fever which was endemic in Bermuda in 1843, which the contagionists traced to an individual, a passenger, landed from a mail steamer, said to have been labouring under fever, but in reality under pulmonary consumption. The fact most in favour apparently of the Bona Vista fever being imported, is the general healthiness of the island for many years previously. But this may be delusive. There are numerous instances in the West Indies, of the absence of the disease for successive years with marked healthiness, and of its appearance suddenly when least expected,—much after the manner of cholera and influenza in various parts of the world."

To what then is to be attributed this persistence in the belief that cholera really possesses contagious properties, which is still so prevalent, among European and other authorities, as entirely to regulate all official action taken in regard to this disease?

There can be no doubt that, in the history of what is called the progress of cholera, whether that may refer to its outbreaks, on land, among bodies of troops in cantonments or on the march; to masses of pilgrims congregated at unhealthy seasons of the year,—or, it may be (in consequence of the authoritative dispersion of a religious fair in which cholera has broken out), streaming back towards their homes, worn down by insufficiency of food, by fatigue, and by terror, and, perhaps, hounded on from station to station, without rest, and by roads which take them away from the means of procuring necessary supplies; or whether it has reference to vessels crowded with native troops on an Indian river, or with pilgrims or emigrants at sea;—under such conditions, many circumstances have been noted, which, at first sight, would really seem to bear out the view that contagion had actually existed, and that the disease had been disseminated in that way, but which, on a closer inquiry into all their bearings, are found capable of a different and far more reassuring interpretation.

When sudden epidemic outbreaks occurring under any of the conditions above named are closely scrutinised, it will be found that certain general features of resemblance are present in all of them, whilst some peculiar, and unavoid-

able conditions superadd something special to a few.

In the first place, in all such aggregations of human beings, those who are attacked by cholera, are seen to be selected from the mass in a manner that can leave no reasonable room for doubt that, whilst a universally prevalent exciting influence has existed, it has been guided to the selection of its victims from among those most susceptible of its action, in consequence of their having been, at the time, in a low state of health, from previous disease, from insufficient nourishment, from intemperate habits, or from mental depression; second, that, on land, where there has been no excessive crowding in confined spaces, the removal of the sick from the general body, and their treatment in what may prove to be, comparatively, more healthy localities, will not be followed by any extension of the disease among those in most close attendance on such sick; third, that in the confined space on board vessels in which the disease may have appeared among any of the classes of persons above enumerated, although there is no reason to believe that it spreads from person to person, yet the atmosphere of such vessels would seem to become so powerfully tainted by some special influence capable of exciting the disease, that, even when it has

begun to cease to excite fresh attacks among the original passengers, who may have, in a measure, as it were, become accustomed to, and less susceptible of, its action, there is great risk of the disease being contracted by persons fresh from shore who may be brought within the range of the emanations pervading such vessels.

As regards the effluvia evolved from Arab vessels whose crowded freights of half-starved pilgrims have been attacked by cholera, it has been recorded, that a stench so fearful has at times been given off from such vessels, as to have compelled the crews of neighbouring craft anchored to leeward of them, to change their positions in order to escape from it.

There can be very little doubt that individuals suffering from cholera who have been landed from vessels situated as above described, have been the means of communicating the disease to others to whose houses they have been removed; but the following considerations must not be overlooked in regard to all such cases:—first, that they are of rare occurrence, and when they do happen, it will generally be found that those so infected have visited their sick friends on board the vessel, on its first arrival; second, that there is no clear evidence that the disease so communicated has ever spread beyond a very limited area,—

that, in fact, as a rule, such cases have only occurred among those who came more immediately in contact with the sick at a time when the impregnation of their clothes, hair, &c., with the peculiar emanations of the vessel had not yet had time to be dispelled; and third, that the arrival of vessels so infected has always been coincident with the presence of a generally prevalent epidemic influence, favourable to the occurrence of the disease, at the port for which the vessel was bound, or at some part of the world in the line of direction proceeding from east to west which, in its advance, this condition usually follows.

It must, moreover, ever be borne in mind that many facts have now been accumulated which tend to show that a vessel at sea may sail into, pass through, and again sail out of a zone of epidemic influence favourable to the occurrence of cholera (or of yellow fever either).

It is therefore possible that a vessel which has been so circumstanced, more especially if it arrives at a westward port, may, although fresh attacks have for some time ceased to occur, still continue to be so tainted by the dangerous emanations previously set up, as to be capable of affecting fresh visitors to the vessel, and of exciting the disease in them in a way that may, at times, have appeared to have thus originated an epidemic

at the vessel's port of destination, although its arrival there may merely have been coincident with that of the advancing epidemic influence which the vessel had encountered on its voyage.

In connexion with such occurrences, it may be well to consider the fact that, in its manifestations over different parts of the world during years of its larger epidemic displays, cholera follows a course which has its general tendency of direction, in regard to the bearings of the countries in which its successive outbreaks declare themselves, which, in the northern hemisphere, is from south-east to north-west. This invariable feature would also, further, go far to connect the general influence in question necessary under special local conditions to favour the development of the disease, with that daily influence on the earth which, in its waves of greatest diurnal intensity, the sun exercises, and which, progressing over the former from east to west, and increasing in intensity with the advance of the seasons, if it does not actually originate them, cannot fail to directly affect terrestrial electro-magnetic conditions, and through their means to exercise, indirectly, a very important control over the vital actions of the human body—more especially as regards those connected with the nervous system, which, of all the parts of the body,

plays the most important part in diseases of this special description—and on the states of soil and atmosphere which, again, cannot but react on health.

This, moreover, has reference not only to the actions excited by the heat which the sun imparts, but also, probably, in a still more marked degree, to those influences which sun-spots, and the electro-magnetic displays with which those are evidently so closely connected, which the more recent scientific discoveries have shown such strong reason for believing that the centre of our system exerts on the earth in common with the rest of its members.

This combined diurnal and seasonal progression of waves of maximum solar influence, it seems reasonable to suppose,—as its mode of progression cannot but be in a general direction from east to west,—must necessarily communicate a similar mode of advance to the local manifestations of the general influence it may excite, in respect of the points of time at which those may declare themselves in different parts of the northern hemisphere of the globe to which the application of the present remarks is confined.

On all the grounds, therefore, which have been mentioned above, it would seem that the most reasonable view to take of the situation produced

by the arrival of a vessel with cholera or yellow fever on board,—or which may recently have passed through such a visitation,—would be to, in some measure, reverse the principle on which such occurrences have hitherto been dealt with, in so far as to consider that it is the inhabitants of the port of arrival who are to be held as being, in the strictest sense of the term, in quarantine with regard to the infected vessel—that is, that no one from the shore should be permitted to go on board such vessel; that it should be anchored apart from others, and carefully fumigated and thoroughly cleansed and ventilated by its own crew; that the sound passengers, after thorough washing of their bodies, and especially of their hair, and assuming clothes that have not been worn or exposed during the voyage, should be permitted to land; that the sick, after similar precautions have been taken in their case, also, should be removed to a well-ventilated hospital; that the baggage of the passengers, and the ship's cargo, after the fumigation of the vessel, should be exposed to the air on deck, and then, when landed, be for a time stored loosely, either in the open air, or in open sheds;—and, lastly, and above all, that the community should be re-assured as to the true bearings of contagion in regard to the whole matter—there being no more powerful auxiliaries to the action of the epidemic influence

favourable to cholera than the mental conditions produced by wearing apprehension, or actual panic. Cholera should, moreover, at such times as those visitations happen to ships at sea, always be looked upon as being, if not then actually present at the port of arrival of vessels so circumstanced, probably, at no great distance off, and liable at any moment to make its presence felt.

From the above, therefore, it will be seen that, in the writer's opinion, a species of *spurious contagious property*, altogether different from that associated with the true contagious diseases,—such as small-pox, &c.,—may, under peculiar circumstances, become grafted on cholera, the disease itself being still, however, no more than ever, communicable by the sick themselves; but that the excessive quarantine rules now enforced on a different view of the nature of this disease, are unnecessary; that they tend, by causing panic, to aggravate epidemics; and that their enforcement is more restrictive of commerce than the circumstances of the case require.

What has just been said in regard to cholera, is to be considered as, in all respects, equally applicable to the allied species, yellow fever.

It has hitherto been held to be a very conclusive argument in favour of cholera being

possessed of the power of advancing by means of personal contagion, from country to country—and notably from India to Europe—that it never fails to do so along the great routes of land traffic. Before, however, any weight can be attached to the fact that cholera may be seen to specially manifest itself along such lines of communication, it must ever be remembered, that in eastern lands there is, from the physical characters of the country, and more especially in consequence of a limited water-supply, generally only one such route; and that, for much of the way, all to right and left of it may be barren and uninhabited.

The appearance of cholera along such a highway in a year when there has been a special and extended prevalence of the disease, comes, therefore, really to mean nothing more than that, at different parts of its course in succession,—and in a general direction from India north-westwards towards Europe,—as the advancing wave of epidemic influence may have arrived there, the disease will manifest itself simply because there alone does it find subjects on whom its power can show itself—there being, probably, no other human beings in that region for it to attack. In this way the disease has come to appear to be advancing by means of personal communication, whereas, there is every

reason to believe, that, in reality, it is the epidemic influence that is travelling onwards and merely attacking such subjects—those most prone to receive its influence—as may happen to be, at the time, present on the sections of its line of progression. As, also, those lines of route are constantly being used, an epidemic wave can never advance towards Europe without making itself thus felt; and in this way, probably, the delusion as to the propagation of the disease by personal contagion is kept up.

As some parts of India, more especially the Delta of the Ganges, are never free from cholera, and as the disease is just as fatally violent in its symptoms at one time as at another, if personal communication were its regular means of diffusion, considering the amount of traffic which goes on, at all times, between different parts of India, and between that and other countries, there would be no cessation of its ravages.

That zones of epidemic prevalence of cholera occur on land, and that they, moreover, advance in a generally determinate direction with the advance of the season, in accordance with what has already been said on this point, the history of the appearance of the disease at distant stations in India, during epidemic seasons, and the evidence already supplied at an earlier part of this essay

with special reference to occurrences in the Punjab, sufficiently prove. Were any further assurance on this point wanting, the following facts recorded at page 646 of the second edition of Dr. Aitken's *Science and Practice of Medicine*, would probably be considered sufficient :—

"4. That the limits of a tainted district are sometimes clearly marked out. In illustration of this, my amiable teacher, the late Dr. W. P. Alison, Professor of Medicine of Edinburgh University, quoted a most striking example in his paper on "exciting causes of epidemics" in *the Medico-Chirurgical Review* for 1854. He wrote (on the authority of Ashton Bostock, Esq., Surgeon of the Guards) that *one* wing of a cavalry regiment just arrived from England, and in high health, ascended the Ganges from Calcutta in boats, there being no cholera at the time in Calcutta. At a certain period of the voyage the troops arrived at a part of the country where cholera prevailed in the villages on the banks of the river, *but with which they did not communicate. Here cases of cholera* occurred in the boats; the men were advised to push on rapidly, and after a few days, when they had passed the limits of the existence of the disease on the banks, it ceased to show itself in the boats. What makes the case peculiarly conclusive is, that the other wing of the regiment followed

afterwards by the same mode of conveyance, became *affected with the disease at the same point, and lost it again at the same point.*"

The second question noted by the writer for consideration,—viz., whether any one special cause of cholera exists, cannot, he feels, fail to have been already answered in the negative by the majority of his readers, after the enumeration of facts and reasonings, submitted to them in the preceding sections of this essay:—in so far, at least, as regards the consideration of those individual accessory causes of the species of the genus "non-contagious fever" (a term, to the use of which the writer would still adhere, in spite of what he has already said in regard to the spurious contagious properties that may become grafted on yellow fever or cholera) which lie outside that great universally prevalent one known as malaria, and which is,—it becomes only more evident, the more closely the nature and history of this class of disease, so closely associated with it, is studied,—the grand fundamental cause of all this special class of complaints—every other exciting or modifying influence outside this special one, being simply accessory and secondary to it—although, at certain times, and under peculiar conditions, now one, and now another of those same secondary causes may come to play parts which

raise them to degrees of importance that can scarcely be over-estimated.

The reasons adduced to prove, on pathological data—and, the writer would trust, on sound physiological reasoning as well—that neither in connexion with enteric fever, with cholera, nor with any of the ague series, is any feature of a truly specific nature manifested, will, he would fain believe, be found to be fairly conclusive on this point. The evidence, moreover, which, after three years of unremitting labour, and most carefully conducted experiments and observations, has been brought forward by Drs. Lewis and Cunningham, to show how completely they have failed to corroborate the statements hitherto so universally credited—and on which all the existing theories of the nature of cholera, and of the mode of its propagation have been based—as to the existence of a fungus, (or indeed of any appearance in cholera discharges which is peculiar to those, and not to be found under other conditions either of health or disease), have given increased force to the writer's arguments. Once, therefore, his views can be accepted, and it can be made clear that neither a germ of typhoid fever, nor a fungus of cholera, has any existence—the bowel-lesions of the former disease, and the discharges of the latter, being seen to be in no way dependent on the

absorption of a specific poison, but affording evidence, simply, of different degrees of intensity of one and the same form of action excited in the intestinal blood-vessels by a peculiar condition which has been lit up in the great nervous centres—then the theory that water is the principal vehicle by which typhoid fever and cholera are disseminated, must fall to the ground from sheer want of the slightest basis on which the supposed fact of its existence could possibly be maintained.

In England, and on the continent of Europe, great stress has been laid upon the poisoning of the water-supply by human excrement, on the ground of a belief in the existence of the specific poisons to which reference has just been made; and whilst there can be no doubt that the use of water so contaminated is most unwholesome, and likely to assist in generating fever which may run on to the typhoid form, or even, under specially favouring epidemic influences, to the higher fever-grade of cholera, the present writer would, nevertheless, maintain that there is no proof whatever that either one or the other of those diseases is produced by the introduction of a specific typhoid fever germ, or of a special cholera fungus in the way alleged; or that the contamination of the water-supply of a community is either the sole cause of disease, to the extent claimed for it, or is

in any way comparable in power, as a direct excitant of it, to the degree of influence possessed in this respect by the conditions of contamination of soil and of atmosphere, on which, in a secondary way, the state of the water-supply, as a general rule, depends.

It cannot but be remarked, therefore, that whilst, on the special view of the nature and cause of those diseases which has hitherto been accepted, the utmost importance has been attached to the condition of the water-supply, little or no weight has been given to that still greater contamination of the soil and atmosphere about human habitations, in which the true danger lies, and of which, as has just been pointed out, the condition of the sub-soil water is only a secondary result.

As has been explained in a previous part of this essay, there is the strongest evidence for believing that from the soil in every part of the world—and, even under ordinary circumstances apart from special contamination of it—there proceed more or less of emanations which, in proportion to their intensity, are injurious to health, and to which, in our ignorance of their real nature, we give the name of malaria.

All sanitary experience, moreover, gathered in relation to the study and treatment of disease, goes incontestibly to prove, that of all known

sources of malaria, that which soil impregnated with human excrement in a state of decay gives out, is the most deadly—but deadly in the sense of being the most intense expression of malaria known; and that, too, altogether apart from its furnishing *specific* germs capable of exciting typhoid fever alone, or cholera alone, according as such germs shall belong to one or other of their two supposed varieties alleged to generate, and to be reproduced by those two forms of disease. In reality—so far as speculation may warrant an expression of opinion as to its nature—malaria would appear to consist of emanations of material particles, capable, by entering the circulation through the lungs and thus reaching the great nervous centres, of exercising an influence on the latter (probably by modifying the nutrition of the substance of the brain and spinal cord) which sets up a condition there that may either, according to circumstances, lead to little or no outward manifestation of disease, or, under the action of still other external influences—and according to the degrees of intensity in which those may exist—may declare itself in any one of the species of the genus of disease in question,—it may be, in all of them in succession,—the milder forms becoming, through neglect or from other causes,

aggravated into their higher degrees of expression.

The large experience enjoyed by British medical officers in the charge of European troops in India—and it is only with such troops that any really valuable practical experience in regard to the diseases of India can be gained—has failed to produce any real evidence in support of the water-theory of the dissemination of cholera. It is, however, curious to note, that the observers belonging to the Indian Medical Service who are the warmest advocates of the mode of propagation in question, are, as a rule, medical officers whose experience in regard to European troops has not been of a practical nature.

It is, also, a most instructive fact, that about the very time (July, 1872) when the one medical officer, who, of all this class, is by far the most ardent believer in the disease-exciting powers of water contamination, was actually engaged, in a lecture delivered at the late meeting of the British Medical Association, in making the following statement in proof of the truth of the theory in question:—"Dugshai, a hill-station, obtains its supply from a spring so situated that it cannot readily become contaminated. There the troops are quite as healthy as they would be in England." To repeat, whilst the above statement was being made,

an epidemic of cholera had in reality begun, which ended by carrying off no fewer than 27 of the troops stationed there, and their families.

The naked truth of the whole matter really seems to be—harsh and presumptuous as it may appear in the writer to make such an assertion—that it is impossible to avoid arriving at the conclusion that the water-theory of the propagation of cholera was originated, and has been maintained, in England and elsewhere, in order to account, on a mistaken view of the pathology of those diseases, for the nature and origin of typhoid fever and cholera.

The writer would, from his own personal observations and experience gained whilst in the medical charge of European troops in India—and he is but little inclined to yield precedence to any one in respect of the earnest desire he has always felt, and the pains he has taken, to collect information bearing on the health of this class—beg leave most cordially to endorse the views so boldly expressed by that very able observer, Inspector-General Dr. Muir, as to the undue importance which has been attached to the influence exercised by the water-supply of the military cantonments of Bengal, as a cause of disease.

With a few exceptions, the water provided for

the troops is, on the whole, good. At Peshawur, it certainly requires improvement; but the present writer has no hesitation in expressing his conviction that, even when all India shall have been furnished with a perfectly pure water-supply, unless other improvements in respect of the conditions of soil, food, shelter, drainage, and conservancy, are made in proportion, any change for the better that may be effected in the general health of the community in respect of the diseases now being specially considered, will be but little perceptible.

As he has already so often repeated, contaminated water may undoubtedly become a source of disease, and the supply of that essential element should, therefore, be of the purest possible description; but that the present water-supply of the British troops in Bengal, is the cause of sickness among them to the extent that has of late been maintained by a certain class of observers, the present writer must re-assert his belief that there is absolutely no proof.

It now only remains for the writer to endeavour to point out what the practical deductions are to which the views recorded in the course of this essay, taken as a whole, lead up, and which may, in their general application,—should they be found really to rest on a basis of truth (and that they

do so, the writer feels confident that time will show),—perhaps, be the means of conferring some lasting benefit on mankind at large.

The one cardinal point in the writer's belief, which, he cannot but think, must form that special basis from which all future sanitary efforts possessing in any degree a right to claim the title to be considered of national importance, must depart, is, that, as population increases, and towns grow in area—and in proportion to the length of time that general stations or particular habitations may have been occupied—so will contamination of the soil increase; and, as a consequence of this, so also will the whole genus of "non-contagious fever," to the consideration of which this essay has been devoted, show itself, not only with increased frequency, but, in all probability, on the occurrence of epidemic influences of even slight intensity, in its higher forms of expression as well. And, be it remarked, that this is said of a particular genus of disease which is, in reality, one that, as regards the world at large, infinitely outweighs in importance every other—not only when considered numerically, but in all other respects besides—and which will in all probability, increase in the frequency and intensity of its ravages, unless special measures are devised to counteract the progressive aggravation of the local causes

from which the varieties of this form of disease will inevitably spring.

Of course, it is scarcely necessary to say that the measures by which the danger foreshadowed above is to be guarded against, must include :—

1*st.* The complete and immediate removal of all human excrement, either on the dry-earth system, or by water through means of covered drains; and its utilization, wherever that may be possible, for the food of plants.

2*nd.* Careful surface cleansing.

3*rd.* The establishment of a pure and abundant supply of water.

On the essential sanitary measures which have just been enumerated, nothing can possibly be added by the writer which has not already been recorded, to far better purpose, by others. There is, however, still one more sanitary expedient not included with the above, which, in Great Britain at least, has—although in no way originally adopted as a sanitary measure—played a most important part in improving the health of the country generally. The measure referred to, is sub-soil drainage properly so called, apart from town drainage meant simply for the removal of surface water, and sewage generally.

The interests of agriculture have already, in a great measure, led to, and will, year by year, still

more thoroughly secure the drainage of all lands brought under cultivation, whenever the nature of the soil at and below the surface is such as to make this necessary.

It is, however, for towns, villages, and the vicinity of human habitations generally, and over and above the usual town drainage works, where the nature of the soil has not enabled the latter to answer the double purpose indicated, that the writer would propose the introduction of thorough sub-soil drainage in addition to sewer drainage, wheresoever the nature of the ground may demand and permit of its employment.

With regard to a remark which has just been made on this subject, the writer would here state that in the porous soil on which Calcutta is built, the system of municipal drains lately constructed has, so far as that has yet extended, completely answered the twofold end of sub-soil as well as sewage drainage by reducing the level of the sub-soil water to that of the town sewers, although none of the water from this source can, where their construction is perfect, enter the latter.

This result may seem strange to those who have never before given much attention to the question of draingae. To the writer, however, the explanation of the occurrence above referred to is easy, as in his youth he became cognisant of an experi-

ment in sub-soil drainage made in the north of Scotland, which appeared, at all events for a time, to have been completely successful,—namely, that of digging deep, narrow drains, and simply refilling them with the earth which the trenches had themselves supplied—the sub-soil water finding a way of escape through the loosened earth which, once set up, seemed, to a certain extent, to go on after the manner of a natural spring flowing along a stratum of more porous earth overlying, or existing as a fault in, one of a more retentive nature.

The trenches of the Calcutta drains act in this way on a large scale—the presence in them of the irregular outer surfaces of the masonry drains, together with the sandy nature of the soil, assisting to secure the continuance of this gratuitous draining process.

In order, however, to give full weight to the proposal which has just been made, and in the hope of demonstrating that the conditions which it is proposed to remedy, and that the manner in which the measure advised could not fail to act in the soils which most call for its adoption, are both matters the sanitary bearings of which are of the first importance, it may be as well to state, in a few words, what those conditions, and what the mode of action of their proposed remedy, are.

There cannot be a doubt that the contamination of ground which is both undrained and uncultivated, is increased progressively, year by year, in porous soil, by the rain-water washing into it all the organic matters which it is either capable of taking up by solution, or of carrying along with it mechanically, and thus tainting both the soil itself and the sub-soil water more and more as time goes on; whilst, annually, during the periods when evaporation through the soil again reduces the level of the sub-soil water raised by the previous rains, the resulting exhalations (vapour of water carrying with it portions of the accumulated organic matter) become, season by season, more injurious in their effects on the human system.

When, however, the soil is not of a porous nature, the neglected accumulations of rain-water, and of organic matters, will remain on, or close to, the surface, and the after-exhalations to which those cannot fail to give rise, will, under such circumstances, probably appear sooner, and, whilst they last, will be likely to act with greater intensity within a given time.

In clayey retentive ground of this description, sub-soil drainage would produce little or no effect, as not much water can penetrate a surface of that description. Careful levelling, and a system of

skilfully arranged, shallow surface drains, such as would ensure the immediate off-flow of rain-water, together with regular surface cleansing, are all that can be done in such circumstances. In many respects, this form of procedure is what is best suited for the peculiar soil of the plains of Bengal, between the highest point to which the Gangetic Delta extends and the Punjab, the surface soil of which, being almost universally a sandy loam, hardens at its surface, under the action of the weather, into a clayey crust through which water does not readily penetrate.

In porous soils, on the other hand,—such as contain a large proportion of sand, and of which the ground Calcutta is built upon supplies a good example,—the action of thorough sub-soil drainage is complete, and the influence exercised by it is a most important one. It is not simply that the adoption of such a measure lowers the sub-soil water, and dries the ground to a level with the depth of the drains employed,—both immediately over those, and laterally so far as the influence of each extends,—but that by causing, during rains, a steady downward flow of the surface water, accompanied by atmospheric air, through the soil, its good effects are secured owing to a progressive washing out, and oxidation, of the organic matters hitherto accumulated in such soils

—a change which cannot fail to exercise a most beneficial effect on the atmosphere of a locality.

The knowledge that such a purifying process attends measures of this description, cannot fail to afford the true explanation of the benefits that sub-soil drainage has conferred on the health of parts of England that were, in former times, far from salubrious.

To what practical end, then, does all that has now been advanced tend;—simply, as regards the civilized world generally, to show, the writer cannot but think, very conclusively, that each Government must endeavour, by well-concerted measures, both to mitigate sanitary evils now existing, and, by anticipation, to ward off those that must, in the future, inevitably accompany increase of population—evils which will bear a proportion to the length of time that any locality, but more especially a densely populated one, may have been occupied.

In this way only can it be hoped to, with any degree of success, keep in check the evolution from the soil—and, in fact, from every one of man's surroundings capable of fostering them—of those emanations (of, it cannot but be inferred, some powerfully active matter, the product of decomposition and decay, and in the most subtile state of division) to which we give the name of malaria.

A fair standard of public health might thus be maintained, through attention to all the collateral circumstances affecting the production and action of such emanations; and in that manner communities might be placed in the best possible position to resist those combined but ever-varying influences to disease of which the intensity will, year by year, in successive solar cycles, flow and ebb in a way that can never be accurately foretold—and that, too, not merely as regards cholera, but in relation, also, to the increase or abatement of every form of that important class of which cholera is only one of the species, and from its lowest to its highest grade of intensity.

Whilst these pages are passing through the press, the report has reached India that it has been affirmed in England, that, owing to increasing sanitary defects in Russia,—and notably in St. Petersburgh,—cholera is becoming localized in that country; and that, it is averred, in consequence of this state of things, England will now be in greater danger than hitherto of suffering from invasions of this disease.

Were cholera a truly contagious disorder, there would certainly be danger of this happening. But, on the view of its nature which the present writer has, he would hope, not without good reason, advanced, he can only see that what has been

called the localization of cholera, will be dangerous to those localities alone that may have fallen into a sanitary state of so bad a description as to have made the fundamental exciting cause of fever and cholera—malaria—so intense in degree, in such situations, as to render the ordinary seasonal and other influences—which would, under more favourable local conditions, have merely excited the lower orders of the scale—under such circumstances sufficient, through their increased intensity, to bring out the higher forms of that class of disease, on nearly all occasions, and under a degree of collateral provocation which, before the existing insanitary pitch had been reached, would only, probably, have led to the manifestation of their milder degrees.

The allegations which have just been referred to in regard to the present state of parts of Russia, ought, therefore, only to be looked upon as warnings not to be disregarded as to what may happen anywhere else, where due heed is not given to ensure the adoption of measures for preventing towns and villages falling into like conditions.

As for the present condition of St. Petersburgh, the writer, after having—thanks to the kindness of that admirable Sanitary Engineer, William Clark—thoroughly investigated the nature and

bearings of the measures so successfully carried out in regard to the water-supply, drainage, and street improvements of Calcutta (between which and St. Petersburgh, both as to level and the nature of the soil, there appears to be a close analogy) cannot but believe that all that is wanted to place the capital of Russia in as fair a sanitary condition as any place liable to so many months of an intense winter can ever be put into, is a sufficient grant of money, and the employment of engineering skill equal to that which has been available for the works of the city in which this essay is being written.

It seems to the writer that he ought not to let the present opportunity pass without offering a few remarks on the subject of the outbreak of fever which has for many months past so persistently ravaged the district of Burdwan; and it appears to him that this stage of his essay is that at which what he has to say on the matter may be most appropriately brought in. He has to observe at starting, that all the evidence bearing on this lamentable outbreak which he has had an opportunity of perusing, and the information he has been able to acquire from those best fitted to express an opinion on it, have only tended to show, very clearly, that the disease in question is merely the ordinary fever of the country, begun

and kept up by the malarial emanations fostered by the water-logged condition into which the sub-soil of the district has fallen.

The condition of the inhabitants would appear, moreover, to have been greatly aggravated by constant relapses of the disease owing to persistence of the original exciting cause, and insufficient medical treatment—it being essential to the latter proving effectual that it shall have been employed at the only period when it can really be of avail in cutting short the disease, namely, at the very outset of the attacks—until, at length, the state of health of the community has fallen so low as to have placed their systems beyond the reach of the powers of medicine unaided by careful nursing and the fullest supply of nourishing and appropriate diet and stimulants, and assisted also by the issue of warm clothing, and of night covering, sufficient to protect them from the effects of the low temperature of the cold season—there being with natives (and indeed with Europeans, also, under like circumstances) no more potent accessory exciting cause of fever, when malaria has already brought the body under its influence, than cold which makes itself painfully felt.

Into so low a condition do the poorer classes, who are the principal sufferers, seem to have fallen, that they appear to be, from inability to

follow their ordinary employments, in a condition bordering on actual starvation; and without the most complete and thoroughly organized scheme of aid (whether official or of a more private nature, or both combined) in the shape of supplies of nourishing food, stimulants, and clothing,—going hand in hand with medical aid,—nothing short of extermination seems to await the majority of the poorer classes in this stricken district.

Without prompt assistance afforded in this complete fashion, the immense sums now being spent principally on pure medical aid, will prove to represent but, to a very great extent, so much waste of the resources of the state, as, by the time that, in the large majority of cases, the disease has been brought under medical treatment, the stage of the disorder has passed at which the pure quinine treatment may be employed with full effect, and that other has set in during which the condition of the sufferer is more likely to be aggravated than otherwise by the free use of this, at other times, invaluable remedy—more especially when the manner in which it is likely to be distributed by the only agency generally available for that purpose is considered.

The fever now prevalent in both the Burdwan and Hooghly districts has been spoken of as being, *par excellence*, what has been called "relapsing

famine fever." The writer has carefully looked into this matter of so-called relapsing fever, and regrets to have to say that he can see in the disease to which this designation has come to be applied, only one of the ordinary forms of the ague series, which, from insufficient treatment at the outset, and persistence of the original exciting cause, tends, as all such fevers do (when it does not at once run into a fatal form), to partially abate and again relapse at periods in days, which, from the outset of the disease, are multiples of seven. In this sense, Burdwan fever is a relapsing fever, but in no other. Neither, the writer is convinced, does any distinct relapsing fever exist. He makes this statement moreover, in the face of his knowledge that for many years past, "relapsing fever" has been, and still is set down as a disease *sui generis*—the writer holding the "relapsing fever" of England, Scotland, and Ireland, to be identical and owning a like origin—due allowance being made for difference of intensity in the exciting cause of the disease in the case of the Burdwan fever, as contrasted with that met with in Great Britain.

As regards the cause of the water-logged condition into which the soil of the Burdwan district would appear to have been for some years past gradually falling, and which must be looked upon

as the root of the whole evil, the writer cannot but think that, whilst a good deal of this may be due to the effects produced by railway embankments in obstructing the leading drainage of the district, and something may also, perhaps, be owing to want of proper care to, year by year, clean out the existing channels, he strongly suspects that a far more important general cause is now exercising, and probably will for years, continue to exert, so powerful an influence on the matter, as to make imperative the construction of leading drainage works of some magnitude, if the district in question is to be maintained in a condition that will render residence in it less dangerous to its inhabitants than it is at the present time.

The geological history of all large Deltas is everywhere identical. In proportion as, from age to age, those deposits increase, the portions of the crust of the earth over which they lie, undergo a certain amount of subsidence under the increasing superimposed weight; and that, too, not only in the immediate locality where it has been applied, but also for some distance laterally, as is being exemplified in the gradual depression now taking place of the Run of Kutch, under the influence of the neighbouring enormous mass of materials brought down, year by year, in the waters of the Indus.

The history of the Gangetic Delta, so far as it can be traced in historic times, would tend to show that the subsidence which its deposit has occasioned, has not taken place in one uniform manner over its whole area, but has been manifested by long periodic oscillations of changes of a more local character—the sinkings happening, at one period, towards the east, again more to the westward, and *vice versâ*.

For many years past, the direction in which the most marked depression would appear to have taken place was towards the eastern side of the Delta. More recently, however, changes that have been observed in the channels of the rivers give strong grounds for inferring, that the change in question affecting the Delta of the Ganges, is tending to become localized more to its western side than formerly.

If this should really prove to be the case,—and as geological changes of this nature are usually slow and progressive,—the probability is, that, unless measures are early adopted, and maintained for years, to, as far as possible, obviate the effects of a progressive lowering of the level of the Burdwan and Hooghly districts, their present condition will become aggravated to a degree that will go far to counteract every effort made to keep its inhabitants in health.

Since the above was written, the writer has seen in reports of the proceedings of the Bengal Government, that, with the forethought and vigour which have of late characterised all its proceedings, by providing for extensive drainage works, the proper steps are being taken to meet the danger which has just been foreshadowed.

In regard to any special bearings which the propositions contained in this essay may have on the maintenance of the health of European troops serving in India, those cannot fail to be, on the whole, so apparent as to render any further remarks upon them unnecessary;—unless in so far as concerns the employment of prophylactic measures in regard to both fever and cholera; and in respect of some observations which the writer has to make on the results of the system now authoritatively enforced in India in regard to the management of troops in times of the epidemic prevalence of cholera.

In regard to the first point, the writer has to remark that the prophylactic use of quinine has already been extensively employed in the Bengal command; and, on the whole, there is evidence to show, with satisfactory results, not only in warding off fever altogether, but, where men fortified by its use have been attacked, in ensuring a milder form of the disease.

The doses employed have been, generally, three grains of the remedy given daily, and for some considerable time, at seasons of the year when fever is most to be dreaded.

The only improvement on this system which the writer would suggest, is the employment at such times of a larger dose—five grains—daily supplied to every individual adult (less to children), but only continued, month by month, for periods varying from 7 to 14 days of each month, according as fever may, at the time, happen to be actually less or more prevalent at the station. He believes that, given in this way, quinine will be more efficacious in the larger doses,—even when administered for a shorter time,—than when used more continuously in smaller quantities; besides preventing unnecessary waste of an expensive and invaluable remedy of which the supply is far from unlimited.

When there is reason to fear that epidemic cholera is about to appear in a station, or when it has actually broken out there, every individual in the garrison should be placed under the above prophylactic treatment, but in this case with the addition, for an adult, of five grains of Dover's powder to each dose of quinine—less of course for a child. This measure should be continued whilst an epidemic lasts, or until all

apprehension of its appearance shall have passed away.

As regards the principles on which the management of troops, during the prevalence of epidemic cholera within the limits of the Bengal command is regulated, the writer feels it to be his duty to express his strong conviction that the almost universal removal into cholera camps, of the portions of garrisons among whom cholera has shown itself, at the most unhealthy seasons of the year, is a measure which is the cause, annually, of many more deaths from cholera than need have happened; besides occasioning an amount of sickness, loss of life, and invaliding from other diseases consequent on the exposure incurred at such times, which is far greater than could well be imagined by any one who has not actually witnessed the evil results which ensue from such movements. That executive medical officers are beginning to feel not quite satisfied that those moves are for the good of the men under their medical charge, may be, in some measure, inferred from the following remarks by the Surgeon of the 37th Regiment, made, during the prevalence of cholera, on a weekly report which recently passed through the present writer's hands. It was written for the week ending 10th August, 1872, is dated from Camp Okara, near Lahore, and is as follows:—

"Frequent moves with the necessary fatigue and exposure to the sun only render the men more susceptible to the disease."

In order, however, to show, that in expressing himself as he has done above, the present writer is not making an unfounded assertion, he would submit the following details in illustration of the point in question.

At Lucknow, in 1869, the writer, for over two months, carried on the duties of the Deputy Inspector-General of Hospitals of that circle, then absent on leave. During the period of his charge, cholera attacked the garrison with much severity, rendering the duties of the post he held both very arduous and more than usually responsible.

The writer's experience of the outset of the epidemic, aided unexpectedly by a peculiarity of circumstances which placed him in a position more than usually favourable for forming an accurate judgment on the matter, satisfied him that a strict observance of the rules laid down by authority for the management of cholera epidemics among European troops, had caused a large mortality; whilst by adopting a different course (which, the necessity for it having once become apparent to him, he did not for one moment hesitate to do entirely on his own responsibility), a result was

obtained, which, even on the least favourable view of the matter—that of comparing only what happened to the part of the 62nd Regiment kept in cantonments, with what befell the portion of the same corps sent into camp—was more favourable to the writer's than to the authorised plan, by nearly four times, in respect of the numbers attacked, and a little over that number, as to deaths.

The actual proportions were as under :—

Attacks in 1,000 of strength, among the 62nd Regiment in cantonments, on the writer's plan	43·7
Ditto ditto among the 62nd Regiment in cholera camp, on authorised plan ...	164·3
Deaths in 1,000 of strength, among the 62nd Regiment in cantonments, on the writer's plan	31·6
Ditto ditto among the 62nd Regiment in cholera camp, on authorised plan ...	136·9

When, however, the results of its working are considered in relation to the whole garrison (no part of which escaped the epidemic), they are found to have been nearly seven times more favourable to the writer's plan, in respect of the numbers attacked, and a fraction over that number, as to deaths.

The actual proportions were as under :—

Attacks in 1,000 of strength, among the

whole garrison of Lucknow, in cantonments, on the writer's plan ... ... 24·1
Ditto ditto among the troops in the cholera camp, on authorised plan ... 164·3
Deaths in 1,000 of strength, among the whole garrison of Lucknow, in cantonments, on the writer's plan ... ... 19·0
Ditto ditto among the troops in the cholera camp on authorised plan ... 136·9

It may be here stated, also, that so greatly did the party (band and two companies of the 62nd Regiment) in the cholera camp suffer from diarrhœa and dysentery, that it became absolutely necessary to recall them to cantonments much within the time after the date of the last death from the disease, fixed for the return of a cholera camp. This, too, was done with the best results.

What the writer's plan referred to above was, will now be shortly explained :—

It consisted, then, in this, that, on the occurrence of a case of cholera in any barrack or building, the inmates were, with the least possible delay, removed to other buildings when available, or into tents pitched near the buildings to be vacated. The purification, lime washing, &c., of the latter were at once ordered, and as those were completed, the buildings were used as

shelter from the midday heat, the tents being then occupied only as dormitories. This was continued for a period of ten days from the date of the last attack. After that time, their full re-occupation was permitted.

By this arrangement, the men were saved the alarm and fatigue of a march into cholera camp; they escaped the fearful heat to which they would have been exposed at midday in tents; they had the advantages of their punkahs and other appliances; and they had their meals in comfort.

This system was quietly, watchfully, but firmly carried out; and with the result that, in no instance, at the worst, did more than three cases in all (including the original one) occur in any one set of men so treated; more frequently there were only two such cases: and it not seldom happened that there was no second seizure after the men were turned out.

The partial re-occupation of the infected buildings within ten days was attended with the best results—a fact showing that the belief that local influences producing cholera are only active during the night, is not a purely speculative one.

The comfort and relief that this system of management proved to the soldiers and their families, as compared with that of removal to cholera camp, is almost beyond belief; and, whilst

contrasting the feelings so inspired, with the moral and physical prostration which assailed the band and companies sent into camp, the writer can liken the state of the latter to nothing which will more appropriately illustrate it, than by comparing their condition (making allowances for difference of physical constitution) to that, as he has seen it described, of the cholera-stricken pilgrims as they dispersed from Hurdwar Fair; and he cannot help suspecting—he makes this statement advisedly—that much loss of life has, for years past, been annually occasioned by indiscriminate removals into cholera camp, which a more intelligent management, adopted from the outset of the epidemic, and carefully but fearlessly persisted in, might have rendered unnecessary.

Of course, moves into cholera camp may still, under peculiar circumstances, be necessary; but the writer cannot help believing that, in the instances where such are really necessary—at a station like Peshawur, for example—the move should be made for a time, not only out of the station, but out of the district, on the first warning being given that an epidemic is advancing towards a cantonment in which previous experience may have shown that outbreaks generally manifest unusual severity. The writer has only to add, further,

on this subject, that the 102nd Madras Fusiliers was one of the corps which, on being attacked with cholera, was managed after his plan, and that the officer commanding that regiment, who, in the course of a long service in India, had seen much of cholera epidemics and cholera camps, expressed himself, in the strongest manner, in favour of the system under which the men of his corps had just been managed.

The writer hopes he may be pardoned if, in the face of the success which attended the plan he has just detailed, and with the measures of prophylaxis, and those proposed for the inauguration of a system of treatment for cholera founded on rational, and, he trusts, to some extent, on established principles, he should feel hopeful that some little benefit to his fellow-men may be the result, should the proposals he has brought forward receive, in the future, a fair trial.

It has occurred to the writer that before concluding the main portion of his essay, it might not be unacceptable to his readers if he ventured, in the light of the novel views on fever which he has been striving to enunciate, to offer a few remarks on an illness the doubtful issue of which, for a time, held all England in an agony of suspense.

The view which the writer would take of the

attack through which His Royal Highness the Prince of Wales passed, is, from the opinion he has seen reason to form as to what he believes to be the true nature of typhoid fever, a very simple one, as regards the origin of that instance of this disease: and he would here at once state, that he has altogether failed to see that there exists any good grounds for attributing either the illness of His Royal Highness, or that which unhappily proved fatal to a young nobleman who had been for a time of the Prince's party, to the insanitary state of either Londesborough Lodge or of Sandringham.

There is, as the writer has already endeavoured to show, a far more exact resemblance between the diseases of the Tropics and those met with in England and other temperate countries, than has hitherto been believed; and he has become convinced, from actual observation, that the short febrile attacks so common in England, and which are there usually returned under the head of simple continued fever, until—just as so constantly happens in India, under like circumstances—the designation has to be changed for that of typhoid fever, when, instead of happily proving of an abortive character, and subsiding after only one accession, the disease assumes its more dangerous continued phase and ends in destructive inflammation and ulceration of the over-distended intestinal glands

—To repeat, he has become convinced that such cases are simply examples of ague.

The eastern counties of England, as is well known, have considerably less rainfall, and enjoy a higher temperature, in most years, during the summer and autumn months, than is the case in regard to parts lying to the westward of London —the clouds which the south-west winds bring from the Atlantic to the south-west of England, having, to a great extent, parted with their moisture, and been dissipated, before passing the meridian of the metropolis.

Owing to those somewhat exceptional climatic conditions which they thus come to possess, the comparatively flat and low-lying lands of this eastern portion of the country, present facilities for the development of malaria which are more marked there than in almost any other part of England—the heat being, during certain months of the year (the writer can affirm this from his experience of two years' residence on the eastern shores of Essex) quite semi-tropical in character.

The malarial influence, therefore, thus produced, will be likely, from what we know of the laws that regulate its action, to do its work upon the body during the summer and autumn months, bringing the systems of those most susceptible to its effects into a condition that will only require

the occurrence of a fitting accessory cause to call it, at any moment, into activity.

In India, experience has clearly shown, that at no time is fever more likely to be forced into existence, in those who have been most exposed to the effects of malaria, than in the autumn months when the cold first begins to manifest itself by great alternations of temperature between day and night.

In England, the autumn is spent by a large proportion of the upper classes of society in shooting. In respect of partridge-shooting, as that entails a fair amount of steady wholesome exercise, it proves, as a rule, highly beneficial to health.

The case is, however, somewhat different when, in October and November, the covert-shooting begins. In this there is not, generally, much exercise required—the "guns" spending perhaps much of the day at various posts in damp woods, waiting for the game to be driven up. It is at such times, in raw and cold weather, that a chill is so liable to be induced which may be the means of, within a few hours afterwards, developing an attack of fever belonging to the ague series. This attack is perhaps looked upon merely as a feverish cold coupled with some degree of indigestion, and it may, as so often happens with similar attacks occurring in India, pass off with little more

care than the use of some simple remedy, and a couple of days' rest; or, not having received its proper treatment at the outset, it may run on to its continued form, and end in the enteric lesions which procure for it the name of Typhoid or Enteric fever.

The Prince of Wales had been shooting in November, and, to use the words of *the Lancet*, likely to be well informed on the subject:—"On Monday, Nov. 13th, His Royal Highness was chilly after a day's shooting; he had not, indeed, felt well on leaving home in the morning, but there was nothing noticeable to create suspicion of typhoid fever for four days afterwards. Then it was that headache became a prominent symptom, with other indications of typhoid, and particularly a high temperature of the body."

A consideration of the above facts, and of all the evidence he has seen in the public prints with regard to various collateral circumstances connected with His Royal Highness's attack, induces the writer to take the following view of the origin and progress of the Prince of Wales's illness. On the 12th November, when shooting, His Royal Highness probably got chilled. The following morning (fever brought out in this way by cold, or any other accessory exciting cause, usually first manifests itself about twelve hours

after exposure to such cause) His Royal Highness does not feel well (the initial stage of the disease), and later in the day there is a feeling of chilliness (rigor—the actual onset of the complaint), which would certainly be followed by the usual hot stage, although no mention is made of this in the report which has just been referred to. The nature of the disease is not recognised at its onset, and the treatment appropriate for that stage (quinine employed at once so as to cut short the attack) is not prescribed ; the complaint does not spontaneously stop short after one onset, and in four days' time there is distinct evidence of its having assumed its more severe continued form.

From that time, the course run by the disease is the usual one, ending in the enteric lesions which must inevitably follow on the persistent congestion of the vessels of the intestinal canal which forms the essential part of the disease,—if that condition continues for a sufficiently prolonged period.

Thanks, however, to an excellent constitution, to the tender nursing of loving devotion, and to the skill and attention of His Royal Highness's Physicians, the life of the Heir to the Throne was mercifully spared to a grateful nation.

The explanation which has just been offered in regard to the mode of origin of the Prince of

Wales's illness, is, probably, equally applicable to the manner in which the fatal malady was excited in the nobleman whose attack set in about the same time as did that of His Royal Highness; and the writer must, with every respect for the opinions of the many high authorities who think otherwise, express his own conviction that there do not exist sufficiently good grounds for the belief that either the one or the other of the illnesses which have just been referred to, originated from the insanitary condition of the rooms occupied some short time previously.

Indeed, the writer regrets to have again to repeat in another form, what he has, in principle, already endeavoured to maintain, that the necessity which seems, at present, invariably to arise, in all investigations of this nature, for searching out some flaw in a drain, or defect in a water-supply —even should this latter have been used daily by thousands who have in no way suffered therefrom —is the result of that germ-poison theory of the origin of typhoid fever at present so universally accepted, but which, nevertheless—arrogant as his daring to make such an assertion must seem to be—the present writer cannot but hope that he has succeeded in making clear his belief that this germ theory is, from first to last, unsound, altogether untrustworthy, and likely, if persisted

in, to lead to results which will not be in accordance with the interests of mankind.

Were typhoid fever, in reality, as liable to be produced by slight insanitary conditions of dwellings as the results of the investigations made at Londesborough Lodge would seem to have satisfied some that it is, but few of the inhabited houses in England, provided with sewer drainage and water-pipes, would ever be free from this malady.

The case of the poor groom who died at Sandringham, would appear to have been that of a delicate lad from the colder climate of Scotland, who arrives in a part of the country where the influences tending to excite malarious fever are more active than they are in that from which he came,—and that, too, at a time of the year when those show themselves with greatest force. Before he has had time to become accustomed to the new climate into which he has been brought (and acclimatization is, in its degree, just as necessary for those changing their places of residence from the north to the south of Great Britain, as it is for new arrivals in India—an instance having come within the writer's knowledge where a family with its servants, moving only from a dry, healthy part of the north of Scotland, into a low-lying, distinctly malarious district in

the south of that country, had to pass through quite a year of a severe acclimatizing process, in the course of which some of the servants broke down altogether and had to return to the north); —before he has become acclimatized to his new residence, he is attacked with fever; the nature of the disease is not recognised and checked at the outset; it runs on to its higher form, and, the lad being of a delicate constitution, proves fatal.

As has already been pointed out, attacks of a nature such as the writer believes all those above alluded to, to have been, can, according to his experience, be at once, and infallibly, cut short by the employment, at their very outset, of the appropriate pure quinine treatment; and if sportsmen were instructed to fortify themselves during the early part of the covert-shooting season by taking a five-grain dose of quinine every night at bedtime, for the first week or so of the time, just as cold weather is setting in, the risk of suffering at all from such attacks would be reduced, in all probability, to a minimum.

The writer believes the prophylactic use of quinine against fever of the nature treated of in this essay, to be not less necessary in England than it is in India, and he feels satisfied that, if this measure were systematically adopted for a week or

so at a time, during the trying autumnal months when the cold weather is setting in, and again in spring when the easterly winds prevail, in the cases of children and delicate young people generally, much fever of this particular class which annually occurs in Great Britain would be altogether avoided.

Indeed, the writer cannot but feel convinced, from what he has seen achieved in India through a close adherence to the principles and practice he has recommended and steadily carried out in regard to fever in that country, that were all the febrile attacks of a like nature, which occur with such frequency in the British islands, dealt with after the same uniform simple fashion *at their very outset*, the present heavy amount of annual sickness and mortality from typhoid fever, would be, in all probability, reduced in a very striking manner.

SUMMARY AND CONCLUSION.

THE views which the writer of the foregoing essay has been striving to maintain, and the measures he has been endeavouring to recommend, may, in a general way, be conveniently compressed into the following propositions:—

1*st*.—That within the last forty years, but little real advance has been made towards the elucidation of the nature of fever.

2*nd*.—That the original observations recorded by Sir John Pringle, more than a century ago, in regard to some of the most important points in the etiology of fever, were far more true to nature than the theoretical views which, with a total disregard to that distinguished writer's labours, have since been grafted on the subject of which he wrote with such clearness and force.

3*rd*.—That the truth of the principle laid down, many years ago, by Sir Robert Christison, namely, that "the first appreciable event in the chain of sequences constituting fever is a functional injury of the nervous system," has been clearly established by subsequent experience; and, further, that no attempt that may be made

to explain the true nature of fever, can possibly be crowned with success which does not set out from a clear understanding, and admission of, the truth of the proposition which has just been quoted.

4*th.*—That the very carefully conducted observations and experiments made by Drs. Lewis and Cunningham, appointed to conduct a special inquiry into the nature and causes of cholera, have entirely disproved the accuracy of the view which attributed the occurrence of cholera to the presence in the intestines of a special fungus—those observers having not only failed to discover that there was any special fungus present in cholera discharges, but even to find any germ or other description of body in cholera matters, which was not equally liable to occur also in other conditions.

5*th.*—That, as regards the germ-hypothesis called in to explain the origin of typhoid fever on the supposition that a special poison is essential to the production of this latter disease, no trustworthy evidence has ever been brought forward to show that such typhoid germ has any real existence.

6*th.*—That a careful examination of the records of symptoms considered along with the appearances discovered in the intestines after death, has

made it apparent that the condition of the bowels found in typhoid fever, as well as the discharges from, and appearances noticed in, the intestines in cases of cholera—and of yellow fever, likewise—are simply the natural and inevitable results of a similar condition of the circulation in the vessels of the intestinal canal which occurs with different degrees of intensity in each of those forms of disease, and is consequent on a peculiar state of the great cerebro-spinal nervous centres (the brain and spinal cord) which has been set up in those organs by a cause or causes acting from without, and which re-acts on the blood-vessels within the abdomen by exercising a special influence on the great sympathetic nerve.

7th.—That the full histories of the diseases named above have demonstrated that they are but species of one genus of fever, of which an ordinary accession of ague may be held to represent the simplest form; that all the higher varieties of this genus may be gradually developed from this one primary form; and that there is no evidence whatever to support the view, which has hitherto so firmly held its ground, that any one of the varieties of this description of fever can generate a specific poison-germ—a poison, specific in the sense of its being capable of reproducing the disease by which it was originated, and that only.

8*th.*—That the accumulated experience of years has only served more fully to establish the belief—the earliest propounded as to their cause—that all diseases of the ague series (and the writer believes he has shown sufficient grounds for including typhoid fever, yellow fever, and cholera under this head) owe their origin to the one fundamental cause as yet only known by the general term malaria, having, however, their times of manifestation and forms of intensity of expression determined by various accessory causes,—among which must be included certain peculiar general influences only spoken of, up to the present time, as epidemic conditions.

9*th.*—That strong evidence has been adduced to show that the more ordinary forms of this ague fever-series possess a stage at which it may be considered almost a certainty that quinine, promptly administered, and its use continued sufficiently long, will not only cut short the disease within six-and-thirty hours of its onset, but will, under ordinary circumstances, give complete security against a relapse of the complaint.

10*th.*—That, reasoning from analogy, the treatment which is effectual in one form of this series, should—due allowance being made for their different degrees of intensity—be proportionately beneficial in all; and, that, therefore, a system of

treatment for cholera has been detailed which the writer cannot but feel sanguine may be found, when fairly tried, to possess no small power in combating what has hitherto proved an altogether uncontrollable malady.

11*th.*—That fevers differing only, to some degree, in intensity from those so prevalent in India make up the larger proportion of febrile maladies met with in the British isles, and in other temperate climates; that they may be successfully treated on exactly the same principles as have been laid down by the writer for guidance in respect to the corresponding diseases met with in the tropics; that a system of prophylaxis such as has been recommended in regard to those diseases as they occur in India, is just as applicable, and quite as much required, for England; and that its adoption in the latter country would hold out a reasonable prospect of, in some measure, checking the amount of sickness and mortality from this class of disorders that occurs annually there.

12*th.*—That the sanitary history of the past, and the special evidence as to the condition into which Russia is at present falling as regards the persistence there of epidemic febrile disease in its highest forms of expression, ought to warn Governments of the necessity that exists for legislating, without loss of time, in respect of all the higher essentials

requisite for securing the best possible sanitary condition of communities.

13*th*.—That, in reality, all past experience goes to prove, that neither cholera nor yellow fever possesses contagious properties in the ordinary meaning of that expression; but that, under conditions of the massing together of human beings during the prevalence of a general epidemic influence favourable to the manifestation of those diseases—on land or on boardship, but especially under the latter circumstances—the atmosphere acquires a very marked power of exciting the disease, more particularly in individuals coming fresh under its influence; that individual cases of disease removed, after due precautions in regard to cleanliness and purification, will not be found to have acquired (apart from the adherence to their clothes and persons of the emanations set up and concentrated on board tainted vessels) any power of communicating the disease to others; and that, under all the circumstances of such cases, the writer believes that certain modified relaxations of the rules at present in force with regard to ships infected with either a cholera or yellow-fever taint, might be introduced with perfect safety to communities, and much advantage to mercantile interests.

In conclusion, the writer would beg leave to add that, long after the portion of his essay which treats of the mode of production of animal heat had been finally printed off, he saw in the *Lancet* for 21st December, 1872, under the heading "Paris —from our own correspondent," a short allusion to a communication made, on the same subject, to the French Academy, by M. Claud Bernard. As, beyond that notice, and a similar one that appeared in the British Medical Journal of the same date, the writer is ignorant of what the views of this distinguished observer are, he is unable to say how far his own may be in accordance with those recently submitted to the Academy of Sciences by M. Bernard.

As regards the wish that may possibly arise in the minds of many of his readers, that the treatment which the writer has recommended for cholera should have been fully tried before it was made public, he has to state that, from the nature of his present duties, he has had no opportunity of doing this; but that, even had he been more favourably placed in this respect, he would still have preferred that, with its details clearly laid down, and the principles on which it is based fully explained, the actual testing of its real practical value should fall into other hands than his own.

The writer has now only further to say, that, if even the inquiries that may become necessary to

secure the complete refutation of the views he has advanced, should prove the means of leading to such a clearer knowledge of the important diseases of which his essay has treated as may be instrumental in conferring some ultimate benefit on mankind, he will consider that more than a quarter of a century of his life spent in the public service will not have been altogether without its reward, however much a long series of very trying experiences may have, up to the present time, almos forced him to conclude that his connexion with the army would end far otherwise.

HOUSE OF LORDS LIBRARY

19 October 1966

Dear Mr Poynter,

Thank you for your letter, telling me that you have not got Alexander Smith's book Fever & Cholera and that you would like it for the library. Here it is, with pencilled inscription to regularise the matter! The title-page needs a little sticking, otherwise it seems to be a fine copy. I see now that the author presented it to this library, but I am sure he will not turn in his grave at the thought of its finding a new home.

I will certainly ask for you when next I happen to have occasion to visit the Wellcome H. M. Library.

All good wishes.

Yours sincerely

Christopher Dobson
librarian